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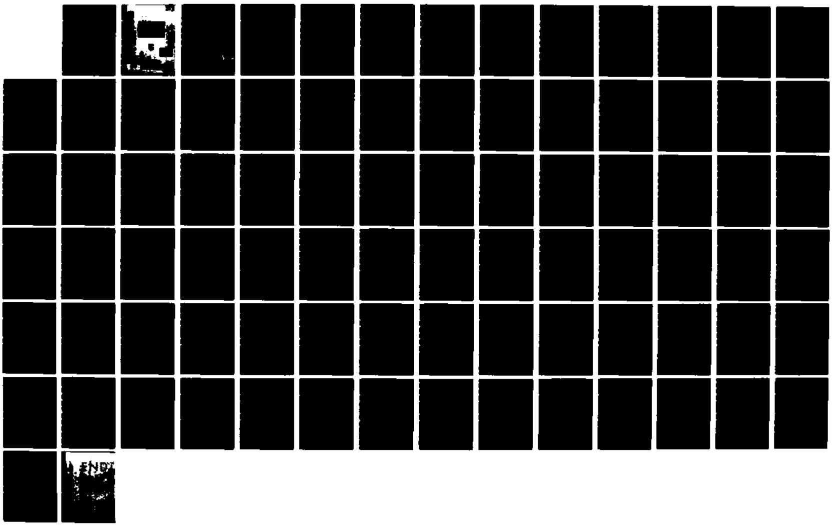


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Ambiguity results from having limited knowledge of the process that generates outcomes. It is argued that many real-world processes are perceived to be ambiguous; moreover, as Ellsberg (1961) demonstrated, this poses problems for theories of probability operationalized via choices amongst gambles. A descriptive model of how people make judgments under ambiguity in tasks where data come from a source of limited, but not exactly known reliability, is proposed. The model assumes an anchoring-and-adjustment

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process in which data provides the anchor, and adjustments are made for "what might have been." The latter is modeled as the result of a mental simulation process that incorporates the unreliability of the source and one's attitude toward ambiguity in the circumstances. A two-parameter model of this process is shown to be consistent with: Keynes' idea of the "weight of evidence," the non-additivity of complementary probabilities, current psychological theories of risk, and Ellsberg's original paradox. The model is tested in four experiments at both the individual and group levels. In experiments 1-3, the model is shown to predict judgments quite well; in experiment 4, the inference model is shown to predict choices between gambles. The results and model are then discussed with respect to: (1) the importance of ambiguity in assessing perceived uncertainty; (2) the use of cognitive strategies in judgments under ambiguity; (3) the role of ambiguity in risky choice; and, (4) extensions of the model.

AMBIGUITY AND UNCERTAINTY IN PROBABILISTIC INFERENCE

The literature on how people make judgments under uncertainty is large, complex, and rife with controversy (see e.g., Edwards, 1954, 1968; Peterson & Beach, 1967; Slovic & Lichtenstein, 1971; Rappoport & Wallsten, 1972; Slovic, Lichtenstein, & Fischhoff, 1977; Einhorn & Hogarth, 1981; Kahneman, Slovic, & Tversky, 1982; Cohen, 1982; Kyburg, 1983). One reason for the controversy is that while there is agreement that "uncertainty" is a crucial factor in inference, there is much less agreement about its meaning and measurement (cf. Tversky & Kahneman, 1982). In particular, while most psychological work on inference has been guided by a Bayesian or subjectivist view of probability, increasing concerns have been expressed about this position (e.g., Cohen, 1977; Shafer, 1978). Central to the Bayesian view is the idea that probability, which is a measure of one's degree of belief, can be operationalized via choices amongst gambles (Savage, 1954). Thus, if two gambles have identical payoffs but one is preferred to the other, it follows that the probability of winning is greater for the chosen alternative.

The subjectivist view of probability gains much of its force by making expressions of uncertainty operational via choices amongst gambles. However, whereas probability is thereby defined precisely, does this procedure capture the essential <u>psychological</u> aspects of uncertainty? In particular, how valid is the assumption that expressions of uncertainty can be captured through choices amongst gambles? An important and direct attack on this assumption was put forward by Daniel Ellsberg (1961) and we examine his arguments below. In doing so, however, we stress that our intent is to understand the psychological bases of uncertainty rather than to critique the normative status of the Bayesian position.

Ellsberg (1961) used the following example to show that the uncertainty people experience has several aspects, one of which is not captured in the usual betting paradigm: Imagine two urns, each containing red and black balls. In urn 1, there are 100 balls but the proportions of red and black are unknown; urn 2 contains 50 red and 50 black balls. Now consider the payoff matrix shown in Table 1. Note that if one bets on red and it is drawn from

Insert Table 1 about here

the urn, one gets \$100; similarly for black. However, if one bets on the wrong color, the payoff is \$0. Imagine you are faced with having to decide which color to bet on if a ball is to be drawn from urn 1; i.e., the choices are red (R_1), black (B_1), or indifference (I). What about the same choices in urn 2; (R_2), (B_2), or (I)? Most people are indifferent in both cases, suggesting that the subjective probability of red in urn 1 is the same as the known proportion in urn 2--namely .5. However, would you be indifferent to betting on red if urn 1 were to be used vs. betting on red using urn 2 (R_1 vs. R_2)? Similarly, what about B_1 vs. B_2? Many people find that they prefer R_2 over R_1 even though their indifference judgments within both urns imply that, $p(R_1) = p(R_2) = .5$. Furthermore, the same person who prefers R_2 over R_1 may also prefer B_2 over B_1. This pattern of responses is inconsistent with the idea that even a rank order of probabilities can be inferred from choices. Thus, if R_2 is preferred over R_1, this implies that $p(R_2) > p(R_1)$. Moreover, since red and black are complementary events, this means that $p(B_2) < p(B_1)$. However, if B_2 is preferred over B_1, then $p(B_2) > p(B_1)$, which contradicts the preceding inequality. It is also important to note that if $p(R_2) > p(R_1)$ and $p(B_2) > p(B_1)$, then either urn 2 has complementary probabilities summing to more than 1 (super-additivity), or, urn 1 has complementary probabilities summing to less than 1 (sub-additivity).

TABLE 1

Payoff Matrix for Gamble Based on
Drawing from Urns 1 and 2

		Outcome	
		Red	Black
Bet	Red	$100	$ 0
	Black	$ 0	$100

Although Ellsberg did not specifically discuss the non-additivity of complementary probabilities (cf. Fellner, 1961), we shall show that it is intimately related to the effects of different types of uncertainty on probabilistic judgments.

From our perspective, the importance of Ellsberg's paradox lies in the difference in the nature of the uncertainty between urns 1 and 2. In urn 1, whereas one's best estimate of the proportion may be .5, confidence in that estimate is low. In urn 2, on the other hand, one is at least certain about the uncertainty in the urn. While it may seem strange, and even awkward, to speak of uncertainty as being more or less certain itself, such a concept captures an important aspect of how people make inferences from unknown, or only partially known, generating processes. Indeed, the idea of uncertainty about uncertainty has been considered from time-to-time under the rubrics, "second-order" uncertainty and probabilities for probabilities (e.g., Marschak, 1975). However, whereas this concept has received little support amongst subjectivist statisticians (see e.g., de Finetti, 1977), its status as a psychological factor of importance for understanding choice and inference has been demonstrated experimentally (Becker & Brownson, 1964; Yates & Zukowski, 1976). On the other hand, the process by which such second-order uncertainty is used in inference and the factors that affect its use, have not been systematically studied. To be sure, Ellsberg suggested a number of variables that should affect the "ambiguity" of a situation, including the amount, type, reliability, and degree of conflict in the available information. Indeed, he stated that,

> Ambiguity is a subjective variable, but it should be possible to identify 'objectively' some situations likely to present high ambiguity, by noting situations where available information is scanty or obviously unreliable or highly conflicting; or where expressed expectations of different individuals differ widely; or where expressed confidence in estimates tends to be

> low. Thus, as compared with the effects of familiar production decisions or well-known random processes (like coin-flipping or roulette), the results of Research and Development, or the performance of a new President, or the tactics of an unfamiliar opponent are all likely to appear ambiguous. (1961, pp. 660-661).

To specify the concept of ambiguity more precisely, reconsider the urn where the proportion of red and black balls is unknown. From a Bayesian perspective, this situation can be thought of as one in which the judge has a diffuse prior over all possible values of the proportion, p(R). However, imagine that one sampled four balls (without replacement) and got 3 red and 1 black. Note that this result rules out certain values of p(R) and could change one's assessment of other values of p(R). Furthermore, as the sample size increases, one should become more sure as to the actual value of p(R). Therefore, as information increases, ignorance (a uniform distribution), gives way to ambiguity (a non-uniform distribution over all outcomes), which then reduces to a known p(R). However, while it is tempting to equate ambiguity with some statistical measure of the dispersion of the subjective distribution, this is unsatisfactory for the following reason: consider an urn that contains either all red or all black balls but you don't know which. In such a case we can characterize the distribution over p(R) as having half its mass at zero and half at one. Note that the variance or range of this distribution is high, yet, ambiguity is low. The reason is that such a distribution rules out all values of p(R) other than 0 or 1 and is thus close to the case where ambiguity doesn't exist (as in urn II). Therefore, in accord with its dictionary definition, "having two or more possible meanings," ambiguity is a function of the number of alternative parameter values that are not ruled out (or made implausible) by one's knowledge of the situation. Note that this definition is similar to, but not identical with, statistical measures such as variance, range, and the like.

It is important to note that sample size is only one factor that influences ambiguity since other information can affect the probability distribution over the parameter of a stochastic process. Thus, imagine an urn factory where employees color balls by throwing them at two adjacent cans of black and red paint from a distance of 20 feet. Given our knowledge of this process, it seems fair to expect that an urn of 100 balls would not contain extreme proportions of red or black. A second example, due to Gärdenfors and Sahlin (1982), is particularly illuminating on this issue:

> . . . consider Miss Julie who is invited to bet on the outcome of three different tennis matches. As regards match A, she is very well-informed about the two players . . . . Miss Julie predicts that it will be a very even match and a mere chance will determine the winner. In match B, she knows nothing whatsoever about the relative strength of the contestants . . . and has no other information that is relevant for predicting the winner of the match. Match C is similar to match B except that Miss Julie has happened to hear that one of the contestants is an excellent tennis player, although she does not know anything about which player it is, and that the second player is indeed an amateur so that everybody considers the outcome of the match a foregone conclusion. (pp. 361-362).

Note that the amount and type of information in the three situations is quite different, as is the amount of ambiguity (we would argue that match A has the least ambiguity and match B the most). From our perspective, how does the amount and type of ambiguity affect judgments of the probability of winning or losing the match? Would Miss Julie, for example, judge that each player in the three matches has a .50 chance of winning (or losing)?

Our discussion so far has strongly implied that ambiguity is generally avoided since it adds to the total uncertainty of a situation. Indeed, this is explicitly mentioned by Ellsberg (1961, p. 666) in discussing why new technologies will be resisted more than one would expect on the basis of their first-order probabilities. However, this picture is not completely accurate, as is made clear by another example provided by Ellsberg (as quoted in Becker

& Brownson, 1964, pp. 63-4, footnote 4): consider two urns with 1000 balls each. In urn I, each ball is numbered from 1 to 1000 and the probability of drawing any number is .001. In urn II, there are an unknown number of balls bearing any single number. Thus, there may be 1000 balls with number 687, no balls with this number, or anything in between. If there is a prize for drawing number 687 from the urn, would you prefer to draw from urn I or urn II? Note that urn I has no ambiguity and each numbered ball has the same .001 chance of being drawn. Urn II, on the other hand, can be characterized as inducing extreme ambiguity (i.e., ignorance). However, for many people, the drawing from urn II seems considerably more attractive than from urn I, thereby implying that there are situations in which ambiguity is preferred rather than avoided. This is considered in detail later, but we note here that accounting for such shifts is an important criterion for judging the adequacy of any theory of inference under ambiguity.

Finally, the concepts of ambiguity, second-order uncertainty, and the like, have been of concern in theories of inference quite apart from their role in affecting choice. For example, work on fuzzy sets (Zadeh, 1978), Shafer's theory of evidence (1976), Cohen's (1977) attempt to formalize uncertainty in legal settings, and the elicitation of probability ranges (Wallsten, Forsyth, & Budescu, 1983), all contain ideas concerning the vagueness that can underly probabilities. Indeed, statisticians have provided axiomatic systems for trying to formalize probability ranges and rank orders rather than specific values (e.g., Koopman, 1940). Moreover, early work by Keynes (1921) also addressed the notion of ambiguity by distinguishing between probability and what he called the "weight of evidence." He stated:

> The magnitude of the probability. . .depends upon a balance between what may be termed the favourable and the unfavourable evidence; a new piece of evidence which leaves this balance unchanged, also leaves the probability of the argument unchanged.

> But it seems that there may be another respect in which some kind of quantitative comparison between arguments is possible. This comparison turns upon a balance, not between the favourable and unfavourable evidence, but between the absolute amounts of relevant knowledge and of relevant ignorance respectively. (Keynes, 1921, p. 71, original emphasis).

Plan of the Paper

We first examine the underlying structure of a set of problems in which ambiguity is a major factor and note how this structure differs from unambiguous situations. We then devote the major part of the paper to the development and testing of a descriptive model of how people make probability judgments and choices under varying amounts of ambiguity. The model is tested in four experiments at both the aggregate and individual subject levels. The implications of the theory and empirical work are then discussed in relation to: (a) the importance of ambiguity in assessing perceived uncertainty; (b) the use of cognitive strategies in understanding probabilistic judgments under ambiguity; (c) the role of ambiguity in risky choice; and (d) extensions of the model to multiple sources and time periods.

A Model for Studying Ambiguity in Inference

The prototypical inference that we consider involves a judge assessing the likelihood of the occurrence of an event based on reports received from a source of limited reliability. The task can be thought of as having the elements schematically represented in Figure 1. (1) An event occurs; (2) The event is "sensed" by observers (e.g., witnesses to an accident) who,

Insert Figure 1 about here

in principle, can be characterized by levels of sensitivity and bias. However, it is important to emphasize that these levels are unknown to the judge (see 5 below); (3) The observers report what they saw. We denote A* as the

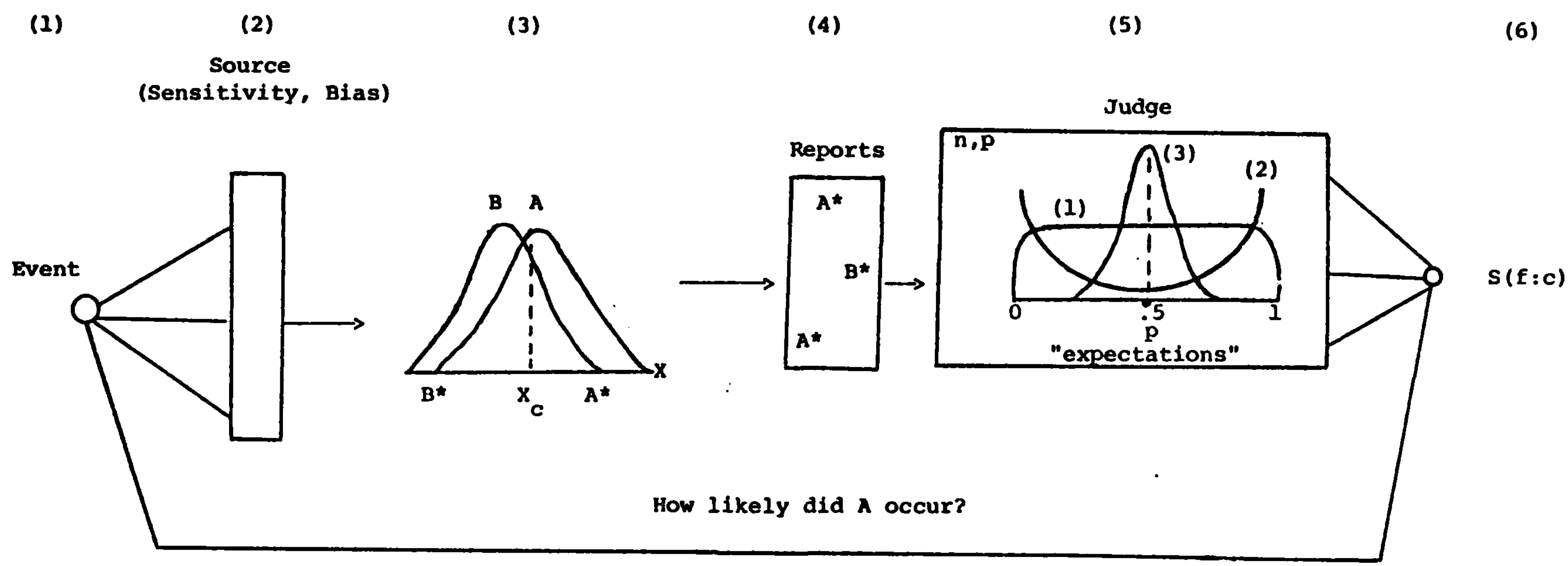

Figure 1. Structure of the inference task

report of event A, and B* as the report of event B, where the decision rule is to report A* if the observation is above some critical value X_c, and B* otherwise. The reports can therefore be conceptualized as coming from a signal-detection task; (4) Since there are n observers, n reports are collected. Thus, the n reports can be thought of as the outcomes of n observers reporting on a single trial of a signal detection task. Furthermore, since we do not differentiate between the n observers, we refer to them as coming from a single source; (5) The judge receives the information in the form of f reports for a hypothesis (i.e., f reports of A*) and c reports of an alternative (i.e., c reports of B*), where $f+c = n$, and $p = f/n$. The content of the scenario, however, is assumed to give the judge some information as to what values of p to expect in a sample of size n. Specifically, we argue that expectations concerning p will be influenced by, (a) the dissimilarity between events A and B; and (b) the credibility of the source. By "credibility" we mean the sensitivity and response bias of the observers in judging the particular events of interest. For example, imagine that you are a detective investigating a bank robbery where two witnesses claim that the robber has blond hair and one witness claims it is brown. How likely does the robber have blond hair? While the detective knows neither the hit and false alarm rates of the witnesses, nor their response bias for saying "blond" vs. "brown," he may know something about the quality of eye-witnesses in a robbery, the confusability of blond and brown in the circumstances, and perhaps something about the motivation of the witnesses. Now contrast this situation where the source is two color television cameras that were filming the robbery at the bank. Whereas in the former case the detective would expect the reports to conflict (i.e., $0 < p < 1$), in the latter it would be surprising if p were not equal to either 0 or 1.

Note that in Figure 1, we have represented the judge's expectations by three different distributions. In distribution (1), the information about the credibility of the source, the dissimilarity of the signals, and the size of the sample, does not rule out many values of p. This is a highly ambiguous situation and would, for example, characterize the detective trying to judge evidence from witnesses. Distribution (2) characterizes expectations based on a highly credible source that discriminates between dissimilar signals; e.g., evidence from cameras filming the robbery. We believe that ambiguity is low here since our knowledge of the process that generates evidence rules out most values of p. Distribution (3) also represents a situation of low ambiguity, but it is quite different from (2). Indeed, (3) is likely to result when the credibility of the source is particularly low and/or the signals are very similar, in direct opposition to the conditions that produce (2). For example, imagine a taste-test between Pepsi vs. Coke for randomly chosen shoppers. If we believe that the two drinks have a very similar taste and that most shoppers are not able to tell the difference, we would expect the proportion of reports for either product to be around .5. Thus, results from such a test might be seen as most closely resembling the drawing of balls from an urn with known $p = .5$. It is interesting to note that whereas some authors have equated increased reliability of evidence with less ambiguity (as suggested by Ellsberg, for example), distribution (3) shows that decreased reliability can also lead to low ambiguity. Another way to express this is to note that high reliability implies low ambiguity (distribution (2)), but low ambiguity does not imply high reliability (since distribution (3) could be involved). As we will show later, both the amount and type of ambiguity (distributions (1), (2), or (3)) affect how probabilistic judgments are made;

(6) The judge combines the information from the reports with expectations

about p to reach an assessment of the likelihood of A.

The structure of this task is both similar to and different from several probabilistic models of the inference process. First, it is similar to cascaded inference in that the judge is making inferences about an event on the basis of unreliable reports (cf. Schum & Kelley, 1973; Schum, 1980). However, in contrast to studies of cascaded inference, the judge does not know the precise value of the source's reliability; rather, there is ambiguity concerning what this is.

Second, since each observer can be thought of as participating in the same signal detection task, the reports not only reflect their sensitivity to competing signals, but also their bias due to differential payoffs. However, as recently emphasized by Birnbaum (in press), the manner in which the judge treats the observer reports depends on some theory about the observers. For example, the observer reports could be responsive to the prior probabilities of A and B as well as to differential payoffs. We emphasize that in our task the judge is not given precise information about these matters. Furthermore, since the judge only receives information on a single trial, the observers' hit-rate and false-alarm rate are not known. Instead, the observed p, and the judge's expectations about p, become cues to the likelihood that the event occurred.

Third, one might consider our situation as a conventional Bayesian revision task (cf. Edwards, 1968). However, the explicit probabilities necessary to assess the likelihood functions are not provided; and, no base-rate data or prior probabilities are stated. It would, of course, be possible to provide the judge with explicit prior probabilities. This would, however, be extending our paradigm to one where multiple sources of information need to be combined, i.e., base-rates and individuating information. For the sake of

simplicity, we only consider the effects of ambiguity on inferences from a single source and thus do not discuss the effects of explicit base rates (extensions of our model to multiple sources is considered in the Discussion section).

Our intent above has been to show how our task is both similar to, and different from, formal models of probabilistic inference. In addition, we note that that although the inference task we consider is quite common, it is difficult to describe it formally when uncertainty cannot be represented by known probabilities. Be that as it may, our purpose is to develop a descriptive model of how inferences under ambiguity are made, and it is to this that we now turn.

A Descriptive Model

We propose that in making judgments under ambiguity, people use an anchoring and adjustment strategy in which the data (reports) serve as the anchor, and adjustments reflect both the amount and type of ambiguity in the situation. We begin by assuming that one has received n reports from some source, with f reports "for" a particular hypothesis and c reports "con" ($n = f+c$). When the judge is asked how likely it is that event A occurred (or, hypothesis A is true), it is assumed that the proportion of reports for A is used as the anchor (i.e., f/n). Note that if the question were reversed, i.e., how likely is it that B occurred, the anchor would be c/n. To model the adjustment process, we posit that people engage in a mental simulation or subjective sensitivity analysis (cf. Fischhoff, et al., 1980; Kahneman & Tversky, 1982) in which outcomes that might have happened are imagined and used for adjusting the anchor.

We model this process in the following way: let $S(f:c)$ be the judged likelihood that some event occurred (or some hypothesis is true) on the basis

of f reports for and c reports against. Furthermore, let k be the adjustment factor, which results as a net effect of simulating both greater and smaller values of the observed p. Thus,

$$S(f:c) = p + k \tag{1}$$

To illustrate the adjustment process, imagine 3 reports from witnesses in which 2 claim that A occurred and 1 claims it was B. The judged likelihood of A is equal to 2/3 plus an adjustment that reflects the unreliability of the reports and the type of ambiguity in the situation. The simulation process is assumed to involve the values of p that might have occurred, but didn't: 3/3, 1/3, 0/3. Clearly, the more unreliable the reports, the more credence is given to the simulation values as opposed to the observed data. Moreover, the simulation is "constrained" by one's prior expectations as to the plausible values of p that are likely in this situation (recall box 5 in Figure 1). Therefore, we conceive of the simulation as reflecting the reliability of the data, which is due to the credibility of the source and the dissimilarity of the signals, and the type of ambiguity in the situation.

The Simulation Process

Since $S(f:c)$ varies between 0 and 1, equation (1) implies that k must be constrained as follows:

$$-p \leq k \leq 1 - p \tag{2}$$

From a psychological viewpoint, this means that the direction of the adjustment must be due, in part, to the value of p. Indeed, when $p = 0$, $k \geq 0$, and the adjustment (if there is one) must be upwards; when $p = 1$, $k \leq 0$, so that the adjustment must be downwards. When $p \neq 0, 1$, one can imagine greater and smaller p's, but the numbers of each are constrained by the particular value of the observed p. In order to model the simulation

process, we consider k to be the net effect of the difference between the number of greater and smaller p's; specifically,

$$k = (k_g - k_s)/n \qquad (3)$$

where, k_g = number of greater p's used in the simulation

k_s = number of smaller p's used in the simulation

n = total number of reports

The difference, $k_g - k_s$, is divided by n since k_g and k_s are numbers of cases, while k must satisfy equation (2). To illustrate how (3) works, reconsider the example of evaluating S(2:1). Note that there is one case of greater p, (3/3), and two of smaller, (1/3, 0/3). If the judge "uses" all three cases and weights greater and smaller cases equally (both of these issues will be discussed below), $k = -1/3$ and the anchor of 2/3 would be adjusted downwards to 1/3.

In equation (3), k_g and k_s are defined as the number of cases used in the simulation rather than the maximum number that could be used. These latter values set an upper limit on the simulation and we consider them first. Thereafter, we discuss: (a) how the unreliability of the data affects the simulation; and, (b) the incorporation of differential weighting for smaller vs. greater values of p. First, consider the constraints imposed on k_g and k_s by the observed p. Specifically, as p increases, the maximum value of k_g decreases and the maximum value of k_s increases. In fact, these values can be written as,

$$\begin{aligned} k_g(\max) &= n(1-p) \\ k_s(\max) &= np \end{aligned} \qquad (4)$$

For example, if one had evidence of (7:3), $k_g(\max) = 3$ (consisting of 8/10, 9/10, 10/10) and $k_s(\max) = 7$. However, as pointed out earlier, the amount

that one simulates will be related to the perceived reliability of the data. To incorporate this into the simulation process, let the parameter θ represent the unreliability of the reports received from the source (larger values of θ indicating greater unreliability). However, since increasing the amount of evidence (n) decreases unreliability, the overall effect of unreliability of the reports can be expressed by,

$$UR = \theta/n \qquad (\theta \leq n) \tag{5}$$

where, UR = overall unreliability of the data $(0 \leq UR \leq 1)$

θ = parameter reflecting the lack of credibility of the source and dissimilarity of the signals

n = number of reports

We can now consider the k_g and k_s used in the simulation as reflecting the maximum values of each as weighted by the overall unreliability of the data; specifically,

$$k_g = \frac{\theta}{n} n(1-p) = \theta\,(1-p) \tag{5a}$$

$$k_s = \frac{\theta}{n} n\,p = \theta\,p \tag{5b}$$

Thus, if the source were perfectly reliable, $\theta = 0$ and there would be no effect for the simulation. Clearly, as θ increases, the range of values used in the simulation also increases.

Up to this point, we have treated greater and smaller values of p as having equal weight or importance in the simulation. This is now rectified by introducing our second parameter, β, which we call one's "attitude toward ambiguity in the circumstances." Since k is the net effect of both k_g and k_s, β only needs to affect one of these components for there to be a

differential weighting effect on k. Thus, we redefine k_s as,

$$k_s = \theta p^{\beta} \qquad (\beta > 0) \qquad (6)$$

We now substitute (6) and (5a) into (3) to get,

$$k = \frac{\theta(1-p-p^{\beta})}{n} \qquad (7)$$

To see the implications of (7), the relations between k, p, and β are illustrated in Figure 2. (In Appendix A, we consider alternative models that result from different weighting assumptions.) First note that k reaches its maximum value of θ/n when $p = 0$ (i.e., where all "might have

Insert Figure 2 about here

beens" must be positive), and its minimum of $-\theta/n$ when $p = 1$ (all "might have beens" must be negative). Moreover, β plays no role when $p = 0, 1$, since differential weight for imagined values of p is not an issue. Second, the figure shows the effects of different levels of β on k; $\beta > 1$ (more weight for k_g than k_s); $\beta = 1$ (equal weight for k_g and k_s); and $\beta < 1$ (more weight for k_s than k_g). An important implication of different values of β is that they affect the value of p for which there is no adjustment (i.e., $k = 0$). Thus, for $\beta = 1$, $k > 0$ when $p < .5$, and $k < 0$ when $p > .5$. In other words, a person with $\beta = 1$ will have upward adjustments for $p < .5$, downward adjustments for $p > .5$, and no adjustment for $p = .5$. When $\beta < 1$, the point of no adjustment, called the "cross-over point" and denoted p_c, occurs below $p = .5$; for $\beta > 1$, the cross-over point is above $p = .5$. In the presence of ambiguity, we expect people to be generally conservative and to give more weight to the possible values below p than to those above it. Thus, we consider $\beta < 1$ to be typical of assessments made under high ambiguity. Conversely, as ambiguity

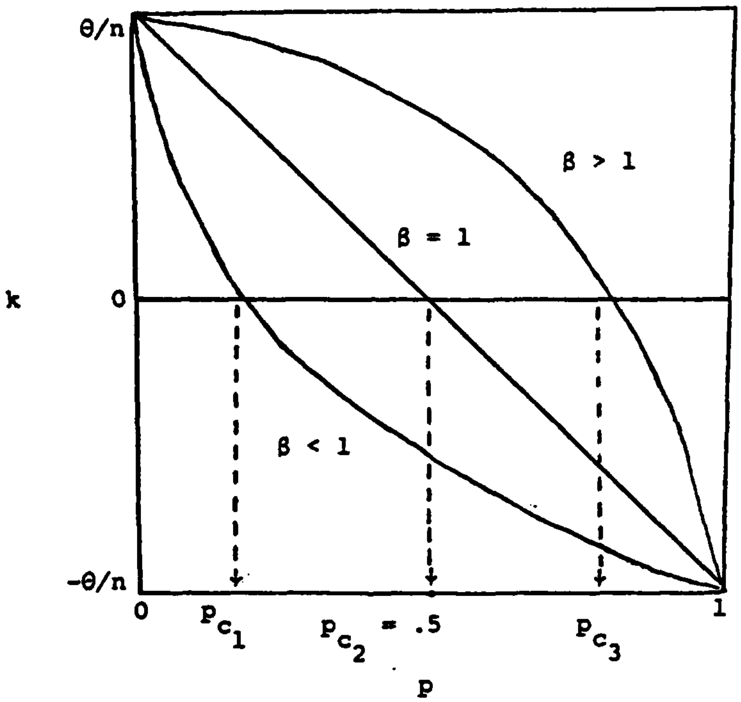

Figure 2. k as a function of p and β

decreases we would also expect people to weight possible values below and above p more symmetrically.

Given the specification of k in (7), the full model is obtained by substituting (7) into (1). This is,

$$S(f:c) = p + \frac{\theta}{n}(1-p-p^{\beta}) \qquad (8)$$

The model in equation (8) has several implications: (1) Consider the effect of the amount of information (n) on judged likelihood. Note that $S \to p$ as $n \to \infty$. This means that as the amount of information increases, one becomes more certain as to the diagnosticity of the data. It is important to realize that as $n \to \infty$, S does not go to 0 or 1 as would be implied by a standard Bayesian revision model. Instead, the fact that S asymptotes at p parallels an analogous result in cascaded inference where, under certain symmetry assumptions, the maximum probability of a hypothesis is bounded by the reliability of the reporting source (Schum & DuCharme, 1971).

(2) Conditional on a given value of θ, the model implies that there will be trade-offs between p and n in determining judged likelihood. For example, one might find the evidence in favor of some hypothesis to be more convincing on the basis of (9:1) than (2:0). However, because S asymptotes at p, trade-offs of p and n will only occur at small values of n. More generally, the model involves trade-offs between four factors: p, n, θ, and β. This is illustrated in Figure 3 which shows how S is "regressive" with

Insert Figure 3 about here

respect to p. First, consider the left-hand panel where p_c is below .5. For given β, the line aa' is determined by θ/n. However, the line aa' becomes bb' if either θ is made smaller or n is increased. That is, θ and n trade-off. Second, consider the right-hand panel in which only one

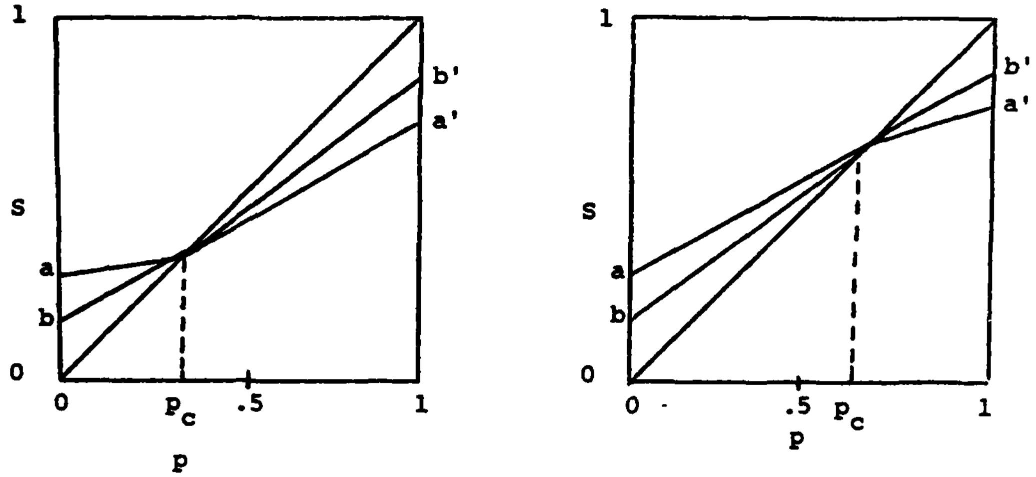

Figure 3. Regressiveness of S on p

parameter has been changed; p_c is now above .5. At the extremes ($p = 0, 1$), S is unaffected by β. However, for $0 < p < 1$, β has a direct impact on S in that S increases with β. Furthermore, note that the effect of Θ and n on judged likelihood is considerably reduced for values of p close to p_c.

(3) What happens when someone judges the likelihood of two complementary events? The sum of these judgments is given by,

$$S(f:c) + S(c:f) = \left\{p + \frac{\Theta}{n}\left(1-p-p^{\beta}\right)\right\} + \left\{(1-p) + \frac{\Theta}{n}\left[p - (1-p)^{\beta}\right]\right\}$$

$$= 1 + \frac{\Theta}{n}\left[1-p^{\beta} - (1-p)^{\beta}\right] \qquad (9)$$

Equation (9) specifies precisely when judgments of complementary probabilities are additive (i.e., sum to 1). Specifically, this occurs when <u>either</u> $\Theta = 0$, $p = 0, 1$, or $\beta = 1$. Moreover, as $n \to \infty$, the sum of judgments of complementary events approaches 1. If the preceding conditions do not hold, the amount of non-additivity is directly related to Θ, and the type of non-additivity depends on β and its implied p_c. Specifically, if $\beta < 1$, complementary judgments will be sub-additive (i.e., sum to less than 1) since $[1-p^{\beta} - (1-p)^{\beta}] < 0$. However, $\beta > 1$ implies super-additivity since $[1-p^{\beta} - (1-p)^{\beta}] > 0$. The importance of equation (9) is that it makes strong predictions as to when sub- or super-additivity will occur, as well as the extent of these effects. Moreover, these phenomena depend on the reliability of the data as captured via Θ and individual attitudes toward ambiguity in the circumstances, i.e., β. Indeed, as Ellsberg's (1961) work demonstrated, ambiguity can lead to probabilities of complementary events that are non-additive.

The above implications deal directly with inference. However, it is difficult (and may not be desirable) to discuss probability judgments without considering their relation to choices under uncertainty. Indeed, we began this paper by discussing Ellsberg's paradox and stated that any theory of inference under ambiguity must explain Ellsberg's original result and his later example demonstrating ambiguity preference. In order to do so, we derive a similar expression to equation (8) for capturing the effects of ambiguous probability assessments on choice. We begin by defining $S(p_A)$ as an assessed probability made in an ambiguous situation (e.g., probabilities assessed on red and black in Ellsberg's urn I). Furthermore, we assume that,

$$S(p_A) = p_A + k \tag{10}$$

where p_A is a value on which the judge anchors (this could be self-generated or given by another; e.g., in a gambling task), and k is the net effect of the adjustment for ambiguity. Thus, $S(p_A)$ is the result of an anchor, p_A, and an adjustment process that reflects the ambiguity in the situation. As discussed previously, k can be decomposed into k_g and k_s;

$$k = \frac{k_g - k_s}{m} \tag{11}$$

where, m = total number of values of p that could be considered in the simulation.

Denote θ as the amount of ambiguity in the situation and,

$$k_g = \theta\, m(1-p)$$

$$k_s = \theta\, mp^{\beta} \tag{12}$$

Equation (12) simply recapitulates the inclusion of an ambiguity weight θ, and the parameter β, which reflects the differential weight for greater and smaller p's. When (12) is substituted into (11) and (10), we obtain,

$$S(p_A) = p_A + \theta(1-p_A - p_A{}^{\beta}) \tag{13}$$

Equation (13) parallels equation (8), except that n no longer plays any role.

To show how (13) can explain the Ellsberg results, consider Figure 4, which shows $S(p_A)$ as a function of p_A for three separate pairs of values of θ and β. Consider (4a), where $\theta > 0$ and $\beta < 1$. Note that a person

Insert Figure 4 about here

with parameter values in these ranges will "underweight" all p_A above p_C, and "overweight" $p_A < p_C$. This particular pattern explains why most people in Ellsberg's urn example choose the unambiguous urn II; that is, $S(p_A = .5) < .50$. However, note that if p_A is less than p_C, $S(p_A) > p_A$ and one would expect the same person who avoided the ambiguous urn when $p_A = .5$, to prefer the ambiguous urn when p_A is sufficiently low. The pattern of overweighting small p_A and underweighting moderate-to-large p_A also accounts for some otherwise puzzling results of Goldsmith and Sahlin (as reported in Gärdenfors & Sahlin, 1982). They presented subjects with descriptions of either well-known events (e.g., drawing cards from a standard deck), or events about which the subjects had little knowledge (e.g., the likelihood of a bus strike in Verona, Italy next week). Subjects estimated the probabilities of the events and the perceived reliability of their probability estimates. Events with equal probabilities but unequal reliabilities were then used in a lottery set-up. The authors report that,

> . . . for probabilities other than fairly low ones, lottery tickets involving more reliable probability estimates tend to be preferred. (Gärdenfors & Sahlin, 1982, p. 363, our emphasis.)

While the pattern shown in Figure 4a accounts for much data, it does not explain why some people in the Ellsberg task prefer to bet on drawing from the

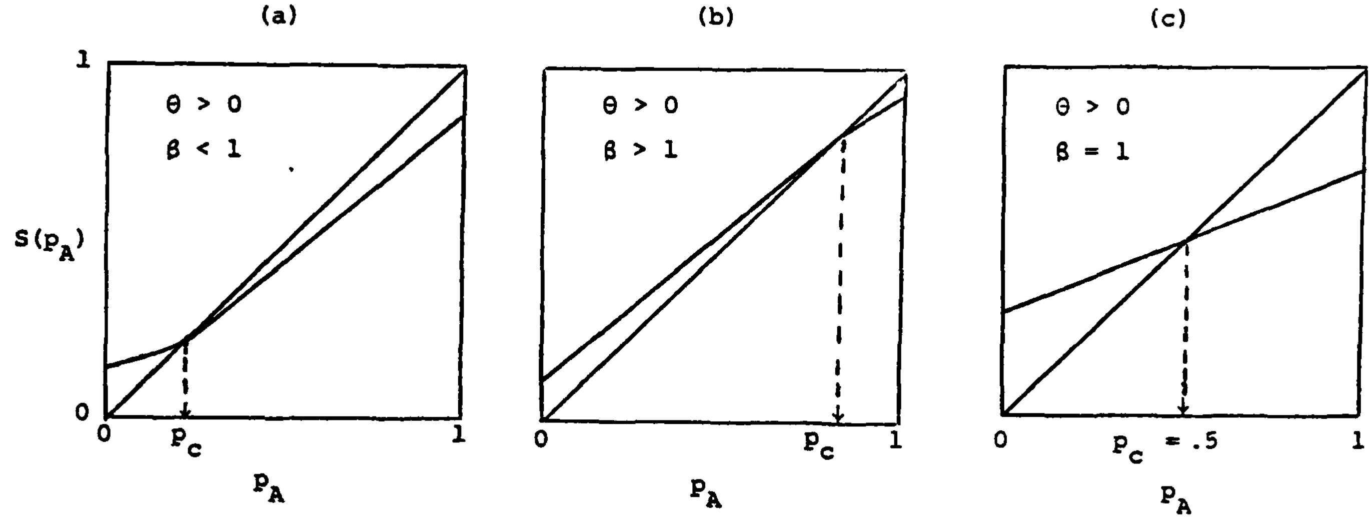

Figure 4. $S(p_A)$ as a function of p_A for values of θ and β

ambiguous urn when $p_A = .5$. However, consider a person with an $S(p_A)$ function as shown in Figure 4b. When $\theta > 0$ and $\beta > 1$, we get "ambiguity preference" over most of the range of p_A. Thus, when $p_A < p_C$, $S(p_A) > p_A$ and over-weighting occurs; when $p_A > p_C$, $S(p_A) < p_A$ and underweighting occurs. Since individual differences are rarely accounted for in research on decisions under uncertainty, our model has the distinct advantage of positing a general psychological process while allowing for individual differences via particular parameter values. Indeed, this is nicely illustrated by considering people who are indifferent between gambles from ambiguous and unambiguous urns when $p_A = .5$ (as in the Ellsberg case). Our model suggests two distinct types: those for whom $\theta = 0$, and thus, $S(p_A) = p_A$; and those for whom $\theta > 0$ and $\beta = 1$ (shown in Figure 4c). This latter group does not adjust at $p_A = .5$, but does adjust at all other values. Therefore, people characterized by these parameter values will only be indifferent between lotteries at .5.

Finally, the model in (13) is relevant to the major psychological theory that examines risk; namely, "prospect theory" (Kahneman & Tversky, 1979). From our perspective, the treatment of uncertainty in prospect theory is consistent with our approach since a decision-weight function is posited that is remarkably similar to the $S(p_A)$ function shown in Figure 4a. This is not a coincidence since, as Kahneman and Tversky specifically point out, decision weights can be affected by ambiguity. Indeed, they state,

> The decision weight associated with an event will depend primarily on the perceived likelihood of that event, which could be subject to major biases. In addition, decision weights may be affected by other considerations, such as ambiguity or vagueness. Indeed, the work of Ellsberg and Fellner implies that vagueness reduces decision weights. (p. 289)

While our equation (13) could be made fully compatible with the decision-

weight function of prospect theory (by restricting its applicability to $0 < p < 1$ and thereby not defining the end points),[1] we wish to emphasize that (13) expresses a class of functions. Therefore, while the decision-weight function of prospect theory expresses a general tendency to treat uncertainty in a particular way, (13) allows for individual differences in the handling of uncertainty.

EXPERIMENTAL TESTS OF THE MODEL

To test our model empirically, we employed a direct inference task (experiments 1-3) and one task dealing with choice (experiment 4). In the direct task, people were asked to make probability judgments on the basis of numbers of reports from a source. In experiment 1, we examined the various implications of equation (8) by considering whether $S(f{:}c)$ asymptotes at p; whether the various parameter values are consistent with the additivity/non-additivity of complementary events, and so on. In experiment 2, the model was tested in different content scenarios, in order to generalize the results from experiment 1. In experiment 3, scenarios that varied in the credibility of the source and the dissimilarity of the signals were used. These allowed us to investigate the effects of the overall reliability of the source on the parameter values of the model. In addition, the consistency of individual differences in strategy (as measured by a person's θ and β parameters) was also considered. The choice experiment involved an attempt to answer the question: Can an individual's choices between gambles be predicted from knowledge of his or her θ and β parameters obtained from a separate inference task? We now turn to experiment 1.

Experiment 1

Subjects. Thirty-two subjects were recruited through an ad in the University newspaper which offered $5 an hour for participation in an experiment on judgment. The median age of the subjects was 24, their educational level was high (mean of 4.4 years of formal post-high school education), and there were 16 males and 16 females.

Stimuli. The stimuli consisted of a set of scenarios that involved a hit-and-run accident seen by varying numbers of witnesses. Moreover, of the n witnesses to the accident, f claimed that it was a green car while c claimed it was a blue car. A typical scenario was phrased as follows:

> An automobile accident occurred at a street corner in downtown Chicago. The car that caused the accident did not stop but sped away from the scene. Of the n witnesses to the accident, f reported that the color of the offending car was green, whereas c reported it was blue. On the basis of this evidence, how likely is it that the car was green?

Each scenario was printed on a separate page and contained a 0-100 point rating scale that was used by the subject to judge how likely the accident was caused by a particular colored car. Each stimulus contained the same basic story but varied in the total number of witnesses (n), the number saying it was a green (f) or a blue car (c), and whether one was to judge the likelihood that the majority or minority position was true. In order to sample a wide range of values of n and p, 40 combinations were chosen as follows: for $p = 1$, $n = 2, 6, 12, 20$; $p = .89$, $n = 9, 18, 27$; $p = .80$, $n = 5, 10, 15, 20, 25$; $p = .75$, $n = 4$; $p = .67$, $n = 3, 6, 9, 12, 15, 18, 24$; $p = .60$, $n = 5, 10$; $p = .50$, $n = 2, 8, 12, 20$; $p = .40$, $n = 5, 10$; $p = .33$, $n = 6, 9, 18$; $p = .25$, $n = 4$; $p = .20$, $n = 5, 10$; $p = .11$, $n = 9, 18$; $p = 0$, $n = 2, 6, 12, 20$. In addition, 8 stimuli were given twice to ascertain test-retest reliability. Thus, the total number of stimuli was 48, and they were arranged in booklet form.

Procedure

When the subjects entered the laboratory, they were told that the experiment involved making inferential judgments. Furthermore, it was stated that if they did well in the experiment (without specifying what this meant), it was likely that they would be called for further experiments. Given the relatively high hourly wage, this was thought to provide some incentive to take the task seriously. In order to avoid boredom and to reduce the transparency that judgments of complementary events were sometimes required, subjects were given 4 sets of 12 stimuli and, after completing each set, they performed a different task. All stimuli were randomly ordered within the four sets. Subjects could take as much time as they needed and they were free to make as many (or as few) calculations as they wished. After completing the task, all subjects filled out a questionnaire regarding various demographic variables.

Estimating the Model

To estimate the model from the experimental data, we need to re-write equation (8) and include a random error term to represent judgmental inconsistency; therefore,

$$S(f:c) = p + \frac{\theta}{n}\left(1-p-p^{\beta}\right) + \varepsilon \qquad (14)$$

Equation (14) requires a non-linear estimation technique which was developed in the following way: let $S(f:c)$ be the actual response of the subject and $\hat{S}(f:c)$ be the predicted response from the model. We wish to minimize some loss function (we chose the mean absolute deviation, MAD), by finding values of θ and β such that,

$$\frac{\sum^{N}\left|S(f:c) - \hat{S}(f:c)\right|}{N} = \text{minimum} \qquad (15)$$

This was done by setting up a grid of values of Θ and β and writing a computer program to first compute the MAD for pairs of "coarse" values of Θ and β. Since certain ranges of Θ and β can thus be excluded, the program then considers "finer-grained" values until MAD is minimized.[2] The output from this analysis is a unique set of values for Θ and β that minimizes the desired loss function.

Since the sampling distributions of Θ and β are not known, testing the statistical significance of the model's fit to the data is problematic. We therefore adopted the strategy of comparing the accuracy of $\hat{S}(f:c)$ with that of a model based solely on p. Moreover, since p is the anchor of the assumed process, any difference between the accuracy of p and $\hat{S}(f:c)$ can be attributed to the adjustment process, and thus to Θ and β. We emphasize that this procedure is biased <u>against</u> finding differences between p and $\hat{S}(f:c)$ for two reasons: (a) the model predicts that $S(f:c) \rightarrow p$ as n increases. Thus, since we have included some large values of n to test this prediction, if $S(f:c) = p$, this counts against, rather than for, the model; (b) the model further predicts that $S(f:c) = p$ at the cross-over point, p_c, and will be close to p in the region of p_c. Again, if this occurs, it counts against the model. We take this highly conservative approach to guard against attributing random error in the data to an adjustment process.

Results

Before discussing the major results, recall that for each subject, 8 stimuli were given twice so that test-retest reliability could be assessed. This was done in two ways: (1) the correlation between judgments of the same stimuli, within each subject (N = 8), was computed. The mean of these correlations was .93, with 26 of the 32 coefficients greater than .90; (2) each subject was considered a replication with 8 responses and the correlation

between judgments for identical stimuli, over subjects (N = 256 = 32 subjects × 8 responses), was .91. Clearly, the reliability of the judgments was high, regardless of the computational method.

For a general impression of how well the model fits the data, we first consider an aggregate analysis (individual differences will be considered in detail below). For each of the 48 stimuli, the judgments from the 32 subjects were averaged to form the mean judged likelihood, $\bar{S}(f{:}c)$. This was then used as the dependent variable to be fit by the model. The parameter values obtained from the estimation program were, $\hat{\theta} = .35$, $\hat{\beta} = .10$ (implying $\hat{p}_c = .16$). In addition, the mean absolute deviation of model and data was .020, which is significantly lower than that of the baseline p-model (MAD = .041; $p < .001$ using a Wilcoxon matched-pairs signed-ranks test).

To see whether the implications of the model hold, consider Table 2, which shows $\bar{S}(f{:}c)$ and $\hat{S}(f{:}c)$ for the 48 stimuli. First, does

Insert Table 2 about here

$S(f{:}c) \to p$ as n increases? The data strongly support this when p = 1, .67, .60, .50, .40, and 0. At the values of .75 and .25, n was not varied although the large adjustments do suggest that the expected effect would occur. However, the effect of n is less clear at p = .89, .80, and .33 since there is little initial adjustment at small n. Taken together, these results suggest moderate support for the hypothesis. Second, do p and n trade-off in affecting judged likelihood? The evidence here is quite convincing: e.g., note that $S(8{:}1) = .88 > S(2{:}0) = .85$, $S(10{:}5) = .65 > S(3{:}1) = .63$, $S(1{:}4) = .21 > S(1{:}3) = .20$. Of particular interest is the result that $S(0{:}2) = .16 > S(1{:}8) = .12$. This means that when there is limited evidence, no data in favor of a hypothesis can be judged as stronger evidence *for* that hypothesis than when more evidence is available with mixed

TABLE 2
Fit of the Model for Aggregate Data

n	p	$\bar{s}$	$\hat{s}$
2	1	.85	.84
6	1	.92	.95
12	1	.96	.97
20	1	.95	.98
9	.89	.88	.86
18	.89	.87	.87
(18)	(.89)	(.85)	.87
27	.89	.87	.88
5	.80	.80	.75
(5)	(.80)	(.73)	.75
10	.80	.79	.78
15	.80	.81	.78
20	.80	.80	.79
25	.80	.82	.79
(25)	(.80)	(.80)	.79
4	.75	.63	.69
3	.67	.61	.60
(3)	(.67)	(.59)	.60
6	.67	.62	.64
(6)	(.67)	(.63)	.64
9	.67	.61	.65
12	.67	.64	.65
15	.67	.65	.66
18	.67	.63	.66
24	.67	.66	.66
5	.60	.53	.57
10	.60	.58	.58
2	.50	.45	.43
8	.50	.44	.48
(8)	(.50)	(.47)	.48
12	.50	.47	.49
20	.50	.47	.49
5	.40	.36	.38
10	.40	.39	.39
6	.33	.31	.32
(6)	(.33)	(.29)	.32
9	.33	.27	.32
18	.33	.29	.32
4	.25	.20	.24
5	.20	.21	.20
10	.20	.19	.20
(10)	(.20)	(.18)	.20
9	.11	.12	.12
18	.11	.13	.11
2	0	.16	.16
6	0	.07	.05
12	0	.06	.03
20	0	.04	.02

Note: Numbers in parentheses are for the repeat judgments.

support. Third, an important implication of the model concerns the relation between θ, β, and the additivity of complementary probabilities. Recall from equation (9) that when $\theta > 0$ and $\beta < 1$, $S(f:c) + S(c:f) < 1$, for $0 < p < 1$. To see if sub-additivity exists in the data and is predictable from the model, consider Table 3, which shows both $S(f:c) + S(c:f)$ and $\hat{S}(f:c) + \hat{S}(c:f)$. Note that there is substantial sub-additivity and the

Insert Table 3 about here

model does a reasonably good job of capturing it. In judging the performance of the model in this regard, it is useful to remember that we have gone beyond the qualitative prediction that sub-additivity will be present in the data, to specifying both the amount of the effect and the conditions under which it will _not_ be present. Given these goals, we view the results as supporting our model.

Individual Analyses

Since each subject rated all stimuli, we can fit the model for each person. These results are shown in Table 4. It is clear from the table that

Insert Table 4 about here

there are substantial individual differences in the parameter values and the degree to which the model fits the data (as indicated by the MAD's). When compared with the aggregate analysis, note that the individual models contain considerably more noise (recall that the MAD for the aggregate data is .020). Furthermore, in comparing each subject's model against the baseline p-model, 14 of the 32 subjects showed no significant adjustment process, _as specified by our model_, while 18 did. The reason for the emphasis is that no subject, even those for whom $\hat{\theta} \approx 0$, used a strict p-strategy (i.e., $S(f:c) = p$ for all p and n). Instead, some used p most of the time but occasionally

TABLE 3

Sub-additivity for the Aggregate Data

p	$(1 - p)$	n	Actual $\bar{S}_n(f:c) + \bar{S}_n(c:f)$	Predicted $\hat{S}_n(f:c) + \hat{S}_n(c:f)$
1	0	2	1.01	1.00
1	0	6	.99	1.00
1	0	12	1.01	1.00
1	0	20	.99	1.00
.89	.11	9	1.00	.97
.89	.11	18	1.00	.98
(.89)	(.11)	(18)	(.98)	.98
.80	.20	5	1.01	.95
(.80)	(.20)	(5)	.94	.95
.80	.20	10	.98	.97
.75	.25	4	.83	.93
.67	.33	6	.92	.95
(.67)	(.33)	(6)	(.92)	.95
.67	.33	9	.88	.97
.67	.33	18	.92	.99
.60	.40	5	.89	.94
.60	.40	10	.97	.97
.50	.50	2	.90	.84
.50	.50	8	.88	.96
(.50)	(.50)	(8)	(.94)	.96
.50	.50	12	.95	.98
.50	.50	20	.94	.98

Note: Numbers in parentheses are for the repeat judgments.

TABLE 4

Fit of the Model for Individual Subjects

Ss	θ	β	p_c	MAD	
1	.00	-	-	.051	ns
2	.00	-	-	.062	ns
3	.02	.01	.03	.002	ns
4	.02	.14	.20	.025	ns
5	.02	.20	.24	.040	ns
6	.02	.23	.27	.113	ns
7	.05	.00	.00	.007	ns
8	.10	1.00	.50	.052	ns
9	.11	.11	.17	.025	**
10	.13	.02	.06	.037	*
11	.15	.00	.00	.081	*
12	.17	.04	.09	.069	**
13	.24	.01	.03	.051	ns
14	.24	.21	.25	.031	ns
15	.28	10.90	.84	.051	***
16	.30	60.00	.95	.052	ns
17	.36	.01	.03	.052	***
18	.36	1.00	.50	.030	**
19	.37	.02	.06	.077	**
20	.37	.08	.14	.033	***
21	.37	.12	.18	.010	ns
22	.42	.04	.09	.079	ns
23	.42	.14	.20	.057	ns
24	.44	.06	.12	.027	***
25	.48	.02	.06	.088	**
26	.50	.01	.03	.023	***
27	.55	.02	.06	.046	***
28	.64	.11	.17	.053	***
29	.84	1.50	.57	.070	*
30	.93	.89	.48	.069	***
31	1.34	.01	.03	.089	***
32	1.83	.03	.08	.106	***

* $p < .05$ (Wilcoxon test)
** $p < .01$
*** $p < .001$
ns not significant

adjusted for n at $p = 0$ and 1, while others had no clearly discernible strategy. This helps to explain why the MAD for subjects with $\hat{\theta} < .10$ is not close to zero, as would be expected if they simply used p for making their judgments. Indeed, subject 6 ($\theta = .02$) had the highest MAD of the 32 subjects. Thus, there seem to be idiosyncratic ways of making probability judgments that are not captured by equation (8).

The above should not detract from the fact that a majority of subjects did show a significant adjustment in accord with the theory. We illustrate this by the results of five subjects, each of which represents a different combination of θ and β parameters. This is shown in Table 5. Subject

Insert Table 5 about here

26 illustrates the use of a highly consistent strategy in which downward adjustments are made over almost the entire range of p. Subject 18 also has a consistent strategy involving adjustments, but $\hat{p}_c = .50$, implying that adjustments will be down when $p > .5$, up when $p < .5$, and no adjustments at $p = .50$. The data conform quite closely to this pattern. Subject 15 has a somewhat less consistent strategy of making small upward adjustments over most of the range of p ($\hat{p}_c = .84$). Again, the data are generally consistent with this interpretation. Subject 3 is included for contrast since, as can be seen, there was almost total reliance on p (as would be predicted by the parameter values and low MAD). Subject 32 is shown to illustrate the most extreme and least consistent adjustment process (which was generally downward). As is evident from the data, this subject had difficulty in "controlling" the adjustment process (cf. Hammond & Summers, 1972, on "cognitive control"). This lack of consistency manifested itself in widely different adjustments for the same stimuli as well as illogical judgments. An example of the latter was that evidence of (0:2) was evaluated as <u>stronger</u>

TABLE 5

Fit of the Model for Selected Subjects

		Subject #26		Subject #18		Subject #15		Subject #3		Subject #32	
n	p	s	$\hat{s}$	s	$\hat{s}$	s	$\hat{s}$	s	$\hat{s}$	s	$\hat{s}$
2	1	.80	.75	.70	.82	.92	.86	.99	.99	.30	.09
6	1	.89	.92	.89	.94	.46	.95	.99	1.00	.80	.70
12	1	.97	.96	.90	.97	.95	.98	.99	1.00	.80	.85
20	1	.98	.98	.88	.98	.88	.99	.99	1.00	.70	.91
9	.89	.83	.94	.84	.86	.84	.89	.88	.89	.90	.71
18	.89	.84	.87	.85	.87	.87	.89	.89	.89	.80	.80
(18)	.89	.87	.87	.85	.87	.87	.89	.89	.89	.70	.80
27	.89	.85	.89	.82	.88	.82	.89	.89	.89	.60	.83
5	.80	.77	.72	.80	.76	.85	.81	.80	.80	.60	.51
(10)	.80	.72	.76	.80	.78	.83	.80	.80	.80	.90	.65
10	.80	.76	.76	.80	.78	.84	.80	.80	.80	.60	.65
15	.80	.79	.77	.79	.79	.74	.80	.80	.80	.70	.70
20	.80	.76	.78	.79	.79	.72	.80	.80	.80	.60	.73
25	.80	.76	.78	.80	.79	.85	.80	.80	.80	.90	.74
(25)	.80	.76	.78	.80	.79	.74	.80	.80	.80	.70	.74
4	.75	.57	.66	.65	.70	.72	.76	.66	.75	.30	.41
3	.67	.57	.56	.64	.63	.74	.70	.66	.67	.20	.27
(3)	.67	.61	.56	.65	.63	.75	.70	.66	.67	.20	.27
6	.67	.59	.61	.65	.65	.72	.69	.67	.67	.60	.47
(6)	.67	.57	.61	.65	.65	.72	.69	.66	.67	.30	.47
9	.67	.59	.63	.65	.66	.65	.68	.66	.67	.30	.54
12	.67	.60	.61	.64	.66	.72	.68	.66	.67	.40	.57
15	.67	.62	.65	.65	.66	.73	.68	.66	.67	.70	.59
18	.67	.63	.65	.65	.66	.65	.68	.67	.67	.70	.60
24	.67	.62	.66	.65	.66	.63	.67	.66	.67	.50	.62
5	.60	.52	.54	.60	.59	.67	.62	.60	.60	.60	.39
10	.60	.57	.57	.60	.59	.66	.61	.60	.60	.40	.49
2	.50	.38	.38	.50	.50	.57	.57	.50	.50	.20	.06
8	.50	.42	.47	.49	.50	.53	.52	.50	.50	.30	.39
(8)	.50	.50	.47	.50	.50	.52	.52	.50	.50	.30	.39
12	.50	.47	.48	.50	.50	.54	.51	.50	.50	.30	.43
20	.50	.48	.49	.51	.50	.55	.51	.50	.50	.30	.46
5	.40	.36	.36	.40	.41	.42	.43	.40	.40	.20	.26
10	.40	.40	.38	.40	.41	.34	.42	.39	.40	.50	.33
6	.33	.27	.30	.31	.35	.25	.36	.34	.33	.40	.24
(6)	.33	.30	.30	.34	.35	.38	.36	.34	.33	.20	.24
9	.33	.26	.31	.34	.34	.35	.35	.33	.33	.20	.27
18	.33	.30	.32	.34	.34	.33	.34	.33	.33	.30	.30
4	.25	.25	.22	.25	.30	.24	.30	.25	.25	.10	.15
5	.20	.18	.18	.21	.24	.23	.25	.20	.20	.20	.14
10	.20	.18	.19	.21	.22	.22	.22	.20	.20	.10	.17
(10)	.20	.20	.19	.40	.22	.26	.22	.20	.20	.20	.17
9	.11	.08	.11	.17	.14	.12	.12	.11	.11	.10	.10
18	.11	.08	.11	.15	.13	.12	.12	.12	.11	.10	.10
2	0	.05	.25	.10	.18	.14	.14	.00	.01	.40	.92
6	0	.03	.08	.10	.06	.13	.05	.00	.00	.30	.31
12	0	.02	.04	.10	.03	.14	.02	.00	.00	.20	.15
20	0	.02	.03	.11	.02	.12	.01	.00	.00	.10	.09
		$\hat{\theta} = .50$		$\hat{\theta} = .36$		$\hat{\theta} = .28$		$\hat{\theta} = .02$		$\hat{\theta} = 1.83$	
		$\hat{\beta} = .01$		$\hat{\beta} = 1.00$		$\hat{\beta} = 10.90$		$\hat{\beta} = .01$		$\hat{\beta} = .03$	
		$\hat{p}_c = .03$		$\hat{p}_c = .50$		$\hat{p}_c = .84$		$\hat{p}_c = .03$		$\hat{p}_c = .08$	
		MAD = .023		MAD = .030		MAD = .051		MAD = .002		MAD = .106	

than (2:0) (i.e., .40 vs. .30). The lack of consistency and large amount of adjusting that characterize subject 32 suggested that there might be a positive relation between the size of θ and MAD, over subjects. When we investigated this, the correlation was $r = .46$ ($p < .001$). Thus, there seems to be a connection between the amount of one's adjustment and the ability to execute it consistently.

Our final results concern the additivity/non-additivity of complementary probabilities for individual subjects. To illustrate this, we use the subjects discussed above whose data are displayed in Table 6. The important

Insert Table 6 about here

thing to note is that subject 26 is consistently sub-additive (and this is predicted quite well by the model); subject 18 is generally additive, as implied by $\hat{p}_c = .50$; subject 15 is super-additive, but not consistently so; subject 3 is additive; subject 32 is both highly sub-additive and inconsistent. From our perspective, these results strengthen our interpretation of the θ and β parameters, as well as the general form of the model.

Experiment 2

The purpose of this experiment was to test the model using different scenarios. However, it is not clear that changing the content of a scenario would leave the credibility of the source unchanged. Therefore, rather than trying to match the perceived accuracy of the sources in the new scenarios to the source in the car accident story, we tried to vary the credibility of the reporting source and the dissimilarity of the competing signals. The following scenarios were used: (1) A taste test where people had to identify a soft drink (Coke vs. Pepsi); (2) A bank robbery where witnesses said the robbers spoke to each other in a foreign language (German vs. Italian); (3) An experi-

TABLE 6

Additivity/Non-additivity of Complementary Probabilities

			Subject #26		Subject #18		Subject #15		Subject #3		Subject #32	
p	(1 - p)	n	Σ	$\hat{\Sigma}$	Σ	$\hat{\Sigma}$	Σ	$\hat{\Sigma}$	Σ	$\hat{\Sigma}$	Σ	$\hat{\Sigma}$
1	0	2	.85	1.00	.80	1.00	1.06	1.00	.99	1.00	.70	1.00
1	0	6	.92	1.00	.99	1.00	.59	1.00	.99	1.00	1.10	1.00
1	0	12	.99	1.00	1.00	1.00	1.09	1.00	.99	1.00	1.00	1.00
1	0	20	1.00	1.00	.99	1.00	1.00	1.00	.99	1.00	.80	1.00
.89	.11	9	.91	.95	1.01	1.00	.96	1.02	.99	1.00	.80	.81
.89	.11	18	.92	.97	1.00	1.00	.99	1.01	1.01	1.00	.90	.91
.89	.11	18	.95	.97	1.00	1.00	.96	1.01	1.01	1.00	.80	.91
.80	.20	5	.95	.90	1.01	1.00	1.08	1.05	1.00	1.00	.80	.65
.80	.80	10	.94	.95	1.01	1.00	1.06	1.03	1.00	1.00	.70	.83
.80	.20	10	.96	.95	1.20	1.00	1.10	1.03	1.00	1.00	.80	.83
.75	.25	4	.82	.88	.90	1.00	.96	1.07	.91	1.00	.40	.57
.67	.33	6	.86	.92	.99	1.00	.97	1.05	1.01	1.00	1.00	.71
.67	.33	6	.87	.92	.96	1.00	1.10	1.05	1.00	1.00	.50	.71
.67	.33	9	.85	.95	.99	1.00	1.00	1.03	.99	1.00	.50	.81
.67	.33	18	.93	.97	.99	1.00	.98	1.02	1.00	1.00	1.00	.90
.60	.40	5	.88	.90	1.00	1.00	1.09	1.06	.99	1.00	.80	.65
.60	.40	10	.97	.95	1.00	1.00	1.00	1.03	1.00	1.00	.90	.82
.50	.50	2	.76	.75	1.00	1.00	1.14	1.14	1.00	.99	.40	.12
.50	.50	8	1.00	.94	.98	1.00	1.06	1.03	1.00	1.00	.60	.78
.50	.50	8	.84	.94	1.00	1.00	1.04	1.03	1.00	1.00	.60	.78
.50	.50	12	.94	.96	1.00	1.00	1.08	1.02	1.00	1.00	.60	.85
.50	.50	20	.96	.98	1.02	1.00	1.10	1.01	1.00	1.00	.60	.91
			$\hat{\theta}$ =.50		$\hat{\theta}$ = .36		$\hat{\theta}$ =.28		$\hat{\theta}$ = .02		$\hat{\theta}$ = 1.83	
			$\hat{\beta}$ =.01		$\hat{\beta}$ =1.00		$\hat{\beta}$ = 10.90		$\hat{\beta}$ = .01		$\hat{\beta}$ = .03	

ment where 6 year old children had to identify words flashed on a screen (ROT vs. BED); and, (4) Experts investigating the cause of a fire (arson vs. short-circuit). The scenarios vary in the degree to which one expects the reporting source to be accurate--the least accuracy occurring in scenario (1) and the most in (4). Scenarios (2) and (3) are intermediate in this regard since the sensitivity of the source in the circumstances is questionable although the competing signals are dissimilar. While the above manipulation is useful for exploring the generality of the model across different content, a more systematic experimental manipulation of credibility and dissimilarity will be discussed in experiment 3.

Subjects and Procedures

Thirty-two additional subjects participated in this experiment. Eight subjects were randomly assigned to each scenario condition and they judged the likelihood that one or other position was true. There were 48 stimuli as in experiment 1, and all other procedures were identical.

Results

We consider the fit of the model to the aggregate data in each scenario (i.e., the dependent variable is $\tilde{S}(f:c)$). These results are shown in Table 7. The basic finding is that the model fits these data quite well. Moreover,

Insert Table 7 about here

the results for additivity are exactly what one would expect on the basis of the $\hat{\theta}$ and $\hat{\beta}$ parameters. The most interesting finding (and one that we explore in the next experiment), concerns the differences in $\hat{\theta}$ and $\hat{\beta}$ across scenarios. Consider the "taste-test" scenario first and note that $\hat{\theta}$ is high and the cross-over point is .51. This means that subjects were adjusting their responses toward .50, which makes sense in this situation.

TABLE 7

Fit of the Model in Four Scenarios

	Taste-test	Bank Robbery	Word Recognition	Fire
$\hat{\theta}$	.75	.35	.35	.25
$\hat{\beta}$	1.10	.00	.30	1.40
$\hat{P}_c$	.51	.00	.30	.55
MAD	.026	.036	.026	.025

That is, when the stimuli are highly similar and the source non-expert, one expects data that do not discriminate between hypotheses. Thus, when subjects see results that are discrepant from .50, they "regress" their judgments toward .50. Now consider the "bank robbery" and "word recognition" scenarios. Here, the values are lower and the cross-over points imply that the data tend to be adjusted downward over most of the range of p. Therefore, these scenarios seem to engender a more "conservative" strategy than the taste-test. Finally, the results for the "fire" scenario show the lowest value of $\hat{\theta}$. This is consistent with the view that the source in this scenario consists of experts and should therefore be adjusted least.

Experiment 3

We had two goals in conducting experiment 3. First, we wished to test systematically for the effects of source credibility and signal dissimilarity on the parameters of the model. In accordance with our theory, θ should decrease as source credibility and signal dissimilarity increase. In addition, we hypothesized that β (and thus p_c) would decrease as ambiguity increased; that is, we expected attitudes toward ambiguity to become more conservative with increases in ambiguity. Second, we wished to investigate the importance of individual differences in the way people cope with the ambiguity inherent in our judgment task.

METHOD

Design. Two levels (high/low) of source credibility and dissimilarity of signals were crossed in a 2 × 2 factorial design. In addition, four different content scenarios were constructed that varied on all four experimental combinations (resulting in 16 different stories). Subjects were asked to judge 21 stimuli that varied in p and n (see below) for each of the four

content-distinct scenarios. Thus, each subject initially made 84 probability judgments. However, in order to reduce boredom in the task, subjects made judgments in all four scenarios, with each scenario representing one of the four experiment conditions. For example, subject 1 received scenario A in the high/high condition, scenario B in the high/low condition, and so on. A four-person latin-square was set up so that every scenario appeared an equal number of times in each experimental condition. Finally, since subjects made judgments in one scenario under the high/high condition, the same scenario was also given in the low/low condition (and the order was counter-balanced). In this way, we were able to examine each subject's judgments holding the content of the scenario constant. This part of the experiment required 21 additional judgments, making the total number of responses for each subject equal to 105.

Stimuli. The four content scenarios used involved the car accident from experiment 1, the word-recognition task from experiment 2 and two new stories. These latter scenarios involved determining the name of a play from an excerpt and, the diagnosis of a medical condition. Four versions of each scenario were constructed to reflect different levels of credibility and dissimilarity (e.g., in the word-recognition task, we had 15 vs. 6 year olds and BED vs. ROT as opposed to BED vs. BID). Within each scenario, subjects were given 21 stimuli that reflected the amount of evidence for each hypothesis. The values of the stimuli were different from those used in experiments 1 and 2 in that smaller values of n were used in order to provide more sensitive tests of the model. The stimuli used were: for $p = 0, 1$, $n = 1, 2, 6$; for $p = .125, .875$, $n = 8$; for $p = .2, .8$, $n = 5$; for $p = .25, .75$, $n = 4$; for $p = .33, .67$, $n = 6, 9$; for $p = .67$, $n = 3$; for $p = .4, .6$, $n = 5$; for $p = .5$, $n = 2, 8$.

<u>Subjects and Procedures</u>. Thirty-two subjects participated in this experiment (comprising 8, 4-person latin-squares). Subjects were paid $5 per hour and the task took about one hour to complete. The tasks were presented in booklets and after each series of 21 judgments, subjects were either given a break or another task. At the end of the experiment, a manipulation check was performed on the credibility and dissimilarity induction. Specifically, each subject was asked to rate (using a 0-100 scale) the credibility of the source and the confusability of the signals in all four scenarios. Since each scenario had high and low levels of each factor, the subjects rated credibility and dissimilarity under both conditions. Therefore, subjects made 4 judgments on each of the 4 scenarios.

Results

Before presenting the main results, we note that the manipulation check showed that subjects did, on average, see the "high" credibility versions of the same scenarios as greater than the low (80 vs. 47); and the high dissimilarity signals as less confusable than the low (30 vs. 62). However, it should be noted that, in absolute terms, the low credibility/low dissimilarity conditions were not extreme. Therefore, contrary to our intentions, we failed to induce a situation of low ambiguity in the low/low manipulation similar to the taste-test scenario of experiment 2. (Recall that the latter could be considered low in ambiguity since subjects essentially treated the reports as emanating from a random process with $p = .5$.)

(1) General fit of the model: For each subject in each experimental condition, the model was fit to yield estimates of θ and β (this resulted in 160 models - 32 subjects × 5 models). The fit of the individual models was comparable to that of experiments 1 and 2 (median MAD = .042 over all conditions).

(2) Manipulation of Θ and β: We first consider the effects of the experimental manipulation on the estimated Θ parameters. The appropriate analysis-of-variance ($2 \times 2 \times$ latin-square) was performed using $\hat{\Theta}$ as the dependent variable and the results showed a significant main effect for "credibility" ($p < .001$), no main effect for "dissimilarity," and a three-way interaction of scenario × credibility × dissimilarity ($p < .02$). The results for the main effects are shown in Table 8. The table shows that Θ does

Insert Table 8 about here

increase as the credibility of the source decreases, thereby confirming our prediction. However, there was no effect for dissimilarity, contrary to our prediction. The three-way interaction showed that in two scenarios, the effect of dissimilarity of the signals had a large effect on Θ when credibility was low, while in the other two scenarios, dissimilarity had a large effect when credibility was high. However, it is not clear why this occurred and we do not consider it further.

In addition to the above analysis, recall that each subject also received the <u>same</u> scenario in the high/high and low/low conditions. A comparison of the means of the estimated Θ's in these two conditions also showed a significant difference in the hypothesized direction; i.e., $\bar{\Theta} = .17$ in the high/high condition, $\bar{\Theta} = .29$ in the low/low ($p < .004$ by a paired t-test). Thus, with the exception of an effect for the dissimilarity of the signals, our hypotheses concerning Θ are supported by the experimental data.

We now turn to the results for the β parameter. However, since β is highly skewed, we substitute its corresponding p_c value in the analyses. First, the analysis-of-variance using p_c as the dependent variable only showed a significant main effect for credibility; i.e., $\bar{p}_c = .23$ for low credibility, $\bar{p}_c = .32$ for high ($p < .002$). In addition, when subjects

TABLE 8

Experiment 3 - Mean $\hat{\theta}$ Parameters by Experimental Conditions

Credibility	Dissimilarity: High	Dissimilarity: Low	
High	.17	.20	.19
Low	.31	.29	.30
	.24	.25	

judged the same story in the high/high and low/low conditions, the analysis showed the same effect; viz., $\bar{p}_c = .36$ in the high/high condition, $\bar{p}_c = .25$ in the low/low ($p < .05$, paired t-test). Therefore, as the credibility of the source increased, the cross-over point in the model also increased.

(3) Individual differences: We now consider the following: (a) can subjects be characterized as having a general strategy, as measured by the consistency of their θ and β values, in different scenarios?; (b) is the amount of one's adjustment, as measured by θ, systematically related to the consistency of executing one's strategy?; (c) can individual perceptions of the credibility of the source and the dissimilarity of the signals account for variance in θ and β <u>within</u> each of the experimental conditions?

(a) Recall that for each subject, four different scenarios were given and a model fit to the data in each. Therefore, each subject can be characterized by four θ's, β's, and MAD's. To determine if the parameter values were more alike <u>within</u> a subject than between subjects (this is measured by the intra-class correlation), a one-way repeated analysis-of-variance was performed (32×4) for $\hat{\theta}$, $\hat{p}_c$, and MAD (Winer, 1963, chap. 3). The results showed that for $\hat{\theta}$, $r = .73$ ($p < .001$); for $\hat{p}_c$, $r = .68$ ($p < .001$); and for MAD, $r = .86$ ($p < .001$). These results are particularly striking when it is realized that the four scenarios varied over the four experimental conditions. In spite of this, the results show strong and stable individual strategies in the amount that is adjusted (θ), the direction of the adjustments (p_c or β), and the consistency of executing one's strategy (MAD).

(b) In both experiments 1 and 2, we found a significant positive correlation between $\hat{\theta}$ and MAD. We examined this in experiment 3 and found the same positive relation in three of the four scenarios ($r = .67$, .48, .40,

.10). Thus, our interpretation of Θ as reflecting a cognitive simulation process is strengthened by the generality of this finding.

(c) Since each subject made independent judgments of the credibility and confusability of the experimental stimuli, we were also able to investigate how these judgments related to the Θ and β parameters within experimental conditions. To do so, we first re-analyzed our data with a regression model where Θ was the dependent variable, and the individual ratings of credibility and confusability, together with dummy variables representing the different scenarios, were the independent variables. More precisely, there is a regression equation of this type for each of the four experimental conditions. However, these four equations can be estimated more efficiently as a single model using Zellner's (1962) procedure for "seemingly unrelated" regressions. The multiple R estimated by this procedure was .44 (with an adjusted R of .35). Of the independent variables, there was no effect for either scenarios or confusability. However, all four coefficients for credibility in the different experimental conditions were significant ($p < .02$) and of the hypothesized sign (i.e., a negative relation between Θ and ratings of credibility). When the same regression technique was used with p_c as the dependent variable, similar results were obtained. We interpret these results as strengthening the conclusions drawn from the more standard ANOVA of our study; that is, Θ and p_c are not only affected by different levels of credibility across all subjects, they also covary significantly with individual perceptions of credibility within each of these levels.

Experiment 4

The purpose of this experiment was to answer the following question: Can individuals' choices between gambles be predicted from knowledge of their Θ

and β parameters obtained from a separate inference task? To examine this, subjects were first asked to make judgments as in experiments 1-3 and both θ and β were estimated as before. The subjects were then asked to choose (or express indifference) between 9 pairs of gambles involving the outcome from an urn with known p, versus the occurrence of an event on the basis of unreliable reports. If θ and β are capturing aspects of ambiguity that affect choice, knowledge of these parameters should allow one to predict individual choices in addition to inferences.

Subjects. Twenty subjects, recruited from the University of Chicago community, participated in this study. They were paid $5/hour.

Stimuli. For the inference task, two different scenarios were used: the car-accident story from experiment 1, and, the taste-test story from experiment 2. These were chosen because the θ and β values were quite different in the two cases. In both scenarios, subjects received 40 combinations of p and n that were identical to those used in the previous experiments. The stimuli for the choice task involved one of the following: (a) For those in the car-accident task, a gambling situation was set-up involving the choice between betting that a ball drawn from an urn with known p was green, versus, betting that the car that caused the accident was green based on witnesses' reports of the car color; (b) For those in the taste-test scenario, the choice was similarly between betting that the outcome from an urn was a certain color, versus, betting that the drink was Pepsi-Cola. In both scenarios, subjects were told to imagine that their payoff for being correct would be $10. Thus, the payoffs for the urn gamble and the bet involving the report of some event were equal. Within scenarios, each subject saw 9 pairs of gambles that varied in the proportion of colored balls in the urn and the proportion of reports favoring the particular hypothesis. These proportions

were always the same in the two bets. The exact values of p used in the 9 pairs were: 1, .875, .75, .625, .50, .375, .25, .125, and 0. The number of balls in the urn and the number of reports were held constant at 8.

Procedure. The twenty subjects were randomly assigned to one of the two scenarios. The procedure for the inference task was identical to the previous experiments. After subjects finished the inference task, they were presented with the appropriate choice task. The nature of the two gambles was explained, and subjects were then asked to choose, or indicate indifference, between the gambles. If they were not indifferent, they were also asked to indicate their strength of preference on a 4-point scale (from "little" to "great deal"). After doing this for one value of p, they turned the page and made another choice (and strength of preference rating, if appropriate) at the next level of p. This continued until all 9 pairs had been considered. Therefore, for each subject, there were 9 choices between an unambiguous bet from an urn with known p, versus an ambiguous bet that an event occurred, on the basis of the proportion of favorable reports from an unreliable source.

Results. Since each subject first participated in the inference task, we briefly consider these results before discussing the choice data. As expected, there were marked differences in the θ and β parameters in the two scenarios. The medians for θ and p_c (implied by β) were .13 and .11, respectively, in the car-accident scenario. For the taste-test, the median θ was 1.35 and median $p_c = .45$. Thus, the taste-test scenario induced much adjustment, with a cross-over point near .50, while the car-accident story induced less adjustment but a lower cross-over point.

To compare each subject's choices with predictions from the inference model, the following procedure was used: any combination of θ and p_c implies when and where $S(p_A)$ is greater, less than, or equal to, p_A (see

equation (13)). Thus, for each subject, when $P_A > S(P_A)$, we predicted the urn would be chosen over the bet based on unreliable reports; when $S(P_A) > P_A$, the opposite prediction was made; when $S(P_A) = P_A$, we predicted indifference between the two gambles. Note that when $\Theta = 0$, we always predicted indifference between the gambles since $S(P_A) = P_A$ for all P_A. In Table 9, we show the $\hat{\Theta}$ and $\hat{p}_c$ values for each subject (grouped by

Insert Table 9 about here

scenario), and the number of correct choice predictions by subject.

To evaluate how well the choices were predicted from knowledge of $\hat{\Theta}$ and $\hat{p}_c$, we used a random baseline for comparison; i.e., for each of the 9 choices made by a subject, there were three possible outcomes; urn, report, or indifference. Since the probability of randomly predicting the correct response is 1/3, we computed the probability of getting at least r hits in 9 trials on the basis of chance (using the binomial distribution). This probability is shown in the last column of Table 9. For example, subject 1 was correctly predicted in 8 of the 9 choices; the probability of getting at least this many hits by chance is .001. Thus, we rejected the hypothesis that our predictions for this subject were no better than chance. Using this method for all subjects, it can be seen that 5 of the 10 subjects in the car-accident scenario, and 4 of 10 in the taste-test, are well predicted using a type I error level of .05. If this error level were increased to .15, a majority of subjects (12/20) would be accurately predicted from their inference parameters. In any event, at the aggregate level (over subjects and scenarios), there were 103 hits out of 179 predictions (one response was missing). The probability of getting at least this many hits by chance is miniscule.

Our final results concern the strength of preference ratings. Recall that in addition to choosing between gambles, subjects were asked to rate

TABLE 9

Choice Predictions from Knowledge of $\hat{\theta}$ and $\hat{p}_c$

	Ss	$\hat{\theta}$	$\hat{p}_c$	No. of hits	Prob. (r ≥ hits)
	1	.00	-	8	.001
	2	.00	-	2	.849
	3	.00	-	3	.612
	4	.00	-	4	.341
Car-Accident Scenario	5	.05	.07	7	.008
	6	.10	.74	5	.140
	7	.16	.15	6	.040
	8	.19	.20	6	.040
	9	.63	.02	5	.140
	10	.75	.50	6	.040
	11	.20	.21	7	.008
	12	.24	.07	4	.341
	13	.66	.50	4	.341
	14	.80	.50	1	.970
Taste-Test Scenario	15	.96	.50	5	.140
	16	1.50	.50	7	.008
	17	1.71	.40	7	.008
	18	1.99	.50	5	.140
	19	2.01	.06	9	.000
	20	3.20	.02	3	.612

their strength of preference on a 4-point scale. These ratings supplement our analysis of the choice data in the following way: in each scenario, the number of prediction errors was 38. However, in the taste-test, θ is much larger than in the car-accident scenario. Since θ is directly related to the amount of adjustment to P_A, the differences between $S(P_A)$ and P_A should be larger in the taste-test than in the accident story. Furthermore, the larger the differences, the stronger one's preferences should be since they are further away from indifference (where $P_A = S(P_A)$). We tested this by comparing the mean strength-of-preference ratings in the two stories across the nine levels of p. These results are shown in Table 10. First, note that

Insert Table 10 about here

the means for the taste-test are larger than the car-accident at every level of p. Second, the pattern of means is consistent with the general form of the model in that preferences are strongest at $p = 1$, decrease as p approaches p_C, and then increase again at $p = 0$. Therefore, the strength-of-preference data are consistent with both the difference in the sizes of θ for the two scenarios, as well as the general form of the model.

DISCUSSION

We now discuss the implications of our theory and results with respect to the following issues: (1) the importance of ambiguity in assessing perceived uncertainty; (2) the use of cognitive strategies in probabilistic judgments under ambiguity; (3) the role of ambiguity in risky choice; and, (4) extensions of the model to multiple sources and time periods.

TABLE 10

Means of Strength-of-Preference
for Two Scenarios

p	Car-Accident	Taste-test	Both Scenarios
1	3.1	3.7	3.40
.875	2.5	3.1	2.80
.750	2.0	2.5	2.25
.625	1.6	2.1	1.85
.500	.9	1.7	1.30
.375	1.4	2.1	1.75
.250	1.3	2.1	1.70
.125	.9	2.0	1.45
0	1.8	2.2	2.00
	1.72	2.39	2.05

Ambiguity and the Assessment of Uncertainty

The concept of ambiguity highlights the distinction between one's lack of knowledge of the process that generates outcomes and the uncertainty of outcomes conditional on some model of the process. The fact that there are at least two sources of uncertainty in most situations leads to the irony that one needs a well-defined model to give precise estimates of how much one doesn't know. Indeed, the usefulness of formulating well-defined stochastic processes is in eliminating ambiguity so that outcome uncertainty can be quantified. Thus, when coins are "fair" or random drawings are taken from urns with known p, there is no second-order uncertainty. Furthermore, the conditional nature of uncertainty is implicitly recognized in various attempts to quantify and improve inferential judgments. For example, consider how uncertainty is defined in the "lens model" (Hammond, et al., 1964). In this case, the uncertainty in the environment is measured as the residual variance not accounted for by a well-formulated ecological model. Thus, unexplained variance or uncertainty is conditional on the model of how particular cues combine to form the criterion of interest. Now consider the work of Nisbett and colleagues on trying to improve probabilistic judgments through training (Nisbett, et al., undated; Jepson, et al., in press). They argue that training and experience can allow one to see the underlying structure of real-world problems so that the appropriate model can be used for making better judgments. Thus, the focus of their training is on making various statistical principles (e.g., regression-to-the-mean, law of large numbers, use of base rates, etc.) more obvious in everyday inferences.

While the conditional nature of uncertainty has been implicitly recognized, ambiguity results from its explicit recognition; i.e., by realizing that the "model" is itself subject to uncertainty. Indeed, one

could argue that the cost of urn models, coin-flipping analogies, and the like, is that they can obscure the fact that most real world generating processes are not precisely known. Furthermore, even if a process is well-defined at one point in time, the parameter(s) of the process can change over time, resulting in ambiguity as well as uncertainty. For example, imagine that you have been asked to evaluate the research output of a younger colleague being considered for promotion. Your colleague has produced 11 papers; of these the first 9 (in chronological order) represent competent, albeit unexciting scholarly work. On the other hand, the latter 2 papers are quite different; they are innovative and suggest a creativity and depth of thought absent from the earlier work. What should you do? As someone who is aware of regression fallacies, you might consider the two outstanding papers as outliers from a stable generating process and thus predict regression-to-the-mean. Alternatively, you might consider the outstanding papers as "extreme" responses that signal a change in the generating process; i.e., a new and higher mean. If this were the case, the same general regression model would predict future papers of high quality (regression to a higher mean). If one asks what is the nature of the signaling in this case, it is obvious that the chronological order of the papers is crucial. Indeed, imagine that the outstanding papers were the first two that were written; or consider that they were the second and sixth. Each of these cases suggests a different underlying model and perhaps a different prediction. In any event, the uncertainty associated with any prediction is obviously complicated by the ambiguity regarding the appropriate mean of the regression process.

Cognitive Strategies in Inferences Under Ambiguity

We have assumed that people use an anchoring-and-adjustment strategy in making inferences under ambiguity. However, whereas the term, "anchoring-and-adjustment" is quite general and could encompass a wide range of models (cf. Lopes, 1981; Einhorn & Hogarth, in press), we have been quite specific as to the nature of this process in our tasks. Of greatest interest in this regard is the idea that adjustments are based on a mental simulation in which "what might have been" is combined with "what is" (the anchor). The rationale for this comes from the fact that the evaluation of evidence often involves an implicit comparison process (similar to the perception of figure against ground). Thus, when evaluating the strength of evidence for a particular hypothesis, the evidence that might have been can serve as a convenient contrast case for comparison. Furthermore, since ambiguity implies that multiple models could have produced the observed results, it seems natural to consider that different results could have occurred on the basis of different underlying processes.

The support for the hypothesized anchoring-and-adjustment strategy comes from several sources. First, recall that in our model, the largest adjustments to the anchor occur at small amounts of evidence. Moreover, as n increases, $S(f:c)$ asymptotes at p. The results of experiments 1-3 support this prediction. Thus, the weight of evidence (to use Keynes' term) for "what is," dominates "what might have been" as the absolute amount of evidence increases. Furthermore, the effect of increasing n is to reduce the amount of non-additivity of complementary strengths. Since most of our subjects were sub-additive, our model provides a psychological link to concerns expressed by others regarding the appropriateness of the complementarity of probabilities based on small amounts of evidence (Shafer, 1976; Cohen, 1977). In

particular, Cohen (1977, chap. 3) points out that when one considers an incomplete system, the lower benchmark on provability is not necessarily disprovability, but non-provability. For example, if one has a meager amount of circumstantial evidence supporting a particular theory such that the probability of the theory's truth is .2, does that imply that the theory is false with $p = .8$? One might rather say that the theory is not proven (in a probabilistic sense) as opposed to saying that there is a .80 chance it is wrong. Furthermore, the idea that the complement of statements can lead to "not-proved" rather than "disproved," seems to be deeply imbedded in the Anglo-American legal system. Indeed, in Scottish law, defendants are either found guilty, not-guilty, or "not proven." The last category is reserved for those cases where the evidence is too meager to allow for a judgment of guilt or innocence.

Second, the fact that *non-additivity* results from a shift in the direction of the adjustment process is consistent with other "order effects" due to the use of anchoring-and-adjustment strategies. For example, in a traditional Bayesian revision task, Lopes (1981) found that a change in the order in which sample information was presented affected overall judgments by changing the anchor. Thus, consider having to judge whether samples come from an urn containing predominantly red or blue balls (70/30 in both cases). You first draw a sample of 8 that shows (5R, 3B). Thereafter, you draw another sample of 8 with the result (7R:1B). After each sample, you are asked how likely it is that you have drawn from the predominantly red urn. When the sample evidence is in the order given here, people seem to anchor on the first sample (5:3) and then adjust up for the second (stronger) sample. However, when the order of the samples is reversed, people anchor on (7:1) and adjust <u>down</u> for the weaker, second sample. This effect cannot be accounted for by

assuming that people are using a Bayesian procedure (which treats the two situations as equal), but it does follow from an anchoring and adjustment process in which the anchor is weighted more heavily than the adjustment.

Third, the results of experiment 3 provide important evidence regarding the process assumed to underlie the model. In addition to the fact that the experimental manipulation of source credibility affected θ and β as predicted, two other results were found; a positive correlation between θ and MAD and, the stability of individual differences in parameter values across scenarios. The first result bears directly on the nature of the adjustment process since it suggests that there is a "cost" of engaging in a mental simulation; namely, a concomitant lack of control over one's strategy (Hammond & Summers, 1972). The second result suggests that there may be strong personal propensities in evaluating evidence that transcend the particular content of scenarios. While it is too early to explicate the nature of these individual differences, their existence lends support to the idea that θ and β are capturing important aspects of the process that determines judgments under ambiguity.

While our model accounts for the rather simple inferences we have studied, it also relates to an important class of inferences that result from "surprise." Consider Figure 5, which shows one's expectations for p as a

<u>Insert Figure 5 about here</u>

function of the credibility of the source and the dissimilarity of the signals. First, note that when credibility and dissimilarity are high, one expects p to be very high or low (recall our earlier example of cameras taking pictures of a bank robber). However, imagine that one camera showed the bank robber to be white, and the other showed him to be black. Such a result, where $p = .5$, would be surprising given the credibility of cameras and the dissimilarity of white and

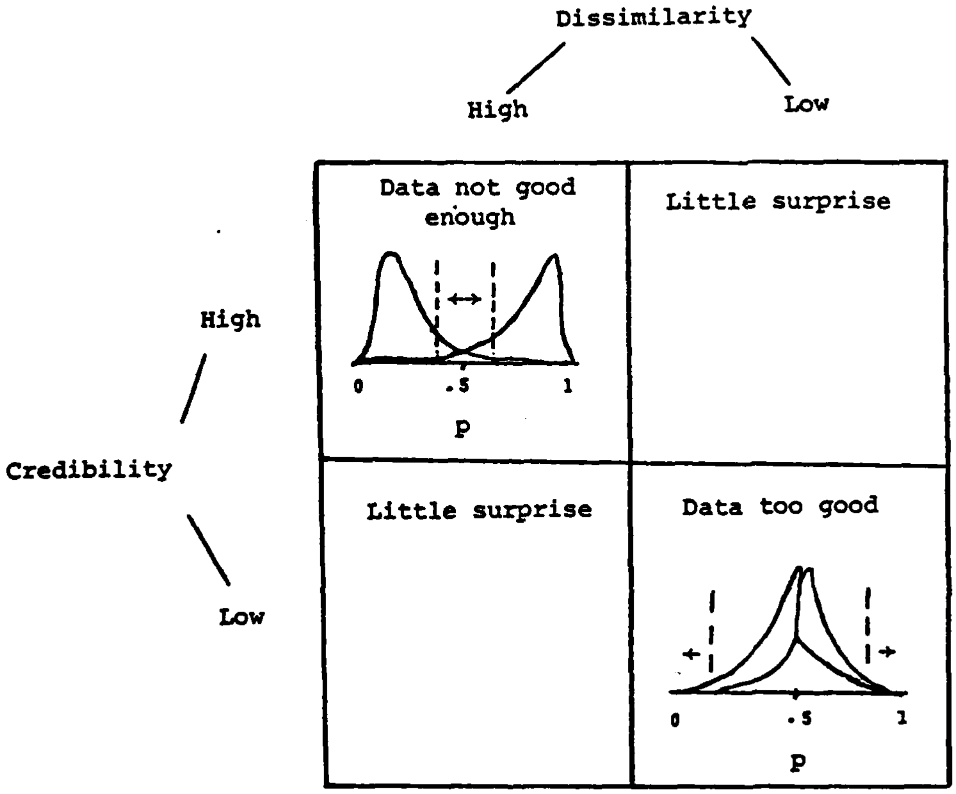

Figure 5. Expectations and surprises

black robbers. Indeed, the data "are not good enough," which is represented by the range of p indicated by the two-headed arrow. Second, consider the low credibility-low dissimilarity situation; e.g., the taste-test scenario. Imagine that you were told that of the 20 people in the Pepsi vs. Coke taste-test, all correctly identified the drink as Pepsi. Such a result, where $p = 1$, would be surprising. However, this type of surprise is one where the "data are too good" rather than not good enough. Thus, there are two types of surprise and both occur when ambiguity is low. Indeed, when ambiguity is high, expectations are weak and surprise (which results from a violation of expectations) is unlikely. This situation characterizes the off-diagonal cells in the figure and accounts for our labeling of these cells, "little surprise."

Although our conceptual scheme makes clear when surprise is likely to occur, it can not handle the variety of possible reactions it can engender. For example, when data are not good enough, it is possible to reduce the credibility of the source (e.g., the cameras were broken), synthesize the hypotheses (there were two bank robbers, one white and the other black), or otherwise make sense of the data by changing the story (e.g., there were two bank robberies on successive days). On the other hand, when data are too good, inferences of fraud, collusion, and the like, are possible (see, e.g., Kamin, 1974 on Burt's twin data; Bishop, Fienberg, & Holland, 1975, on Mendel's pea experiments). An interesting aspect of such inferences is that the surface meaning of the data can suggest the opposite conclusion; e.g., consider someone who "protesteth too much," or a suspect who was "framed" for a crime. Indeed, this is implied by our model. Specifically, consider the case of totally unreliable data, which occurs when $UR = 1$ or $\theta = n$ (see equation (5)). In this case,

$$S(f:c) = 1 - p^{\beta} \qquad (16)$$

Thus, as p increases, S(f:c) decreases. More generally, as UR increases, it will reach a point, conditional on p and β, where the evidence for a hypothesis will be counted against it.

Ambiguity and Risk

Although the importance of ambiguity for understanding risk has been evident since Ellsberg's original article, its omission from the voluminous literature on risk is puzzling. One reason for this omission may be the reliance on the explicit lottery, with stated payoffs and probabilities, for representing risky choice. Indeed, as Lopes (1983) has noted,

> The simple, static lottery or gamble is as indispensable to research on risk as is the fruitfly to genetics. The reason is obvious; lotteries, like fruitflies, provide a simplified laboratory model of the real world, one that displays its essential characteristics while allowing for the manipulation and control of important experimental variables. (1983, p. 137)

It should be further noted that the explicit lottery has been of equal importance to those interested in axiom systems and formal models of risk.

While explicit lotteries have been, and continue to be, useful for studying risk, the ambiguities surrounding real world processes in domains such as nuclear power, environmental safety, and the like, accentuate the incomplete nature of such representations. Indeed, Ellsberg pointed out the particular importance of ambiguity in understanding people's reactions to new technologies (also see, Edwards & von Winterfeldt, 1982, for a historical look at reactions to earlier technological innovations). In any event, the neglect of ambiguity in theories of risk is slowly giving way to interest at both the formal-axiomatic level (e.g., Fishburn, in press, 1983; Gärdenfors & Sahlin, 1982; in press; Morris, 1983) as well as the psychological level (Lopes, 1983).

From the perspective of this paper, the link between inference and choice via ambiguity, provides a unified treatment of uncertainty that has been largely missing from current theories of risk. Moreover, our experimental results, in which choices were predicted from knowledge of the parameters obtained in the inference task, suggest that the process that affects inferential judgments is also present in choices between ambiguous and non-ambiguous options. We should emphasize, however, that we have not provided a theory of risk. In particular, we have not treated the payoff or utility side of the issue. However, we expect that ambiguity will interact with factors such as, whether payoffs are gains or losses (Kahneman & Tversky, 1979), the absolute size of payoffs, and the type of conflict in the gamble. These issues await further research.

Extensions to Multiple Sources and Time Periods

In order to examine inferences under ambiguity in depth, we have restricted ourselves to how evidence from a single source is evaluated at one point in time. However, consider the more realistic situation where decision makers receive information from multiple source-types (including base rates) over multiple time periods. The aggregation of information over source-types and time can be conceptualized by an "evidence matrix" that has source-types for rows and time periods for columns. Such a matrix is shown in Figure 6.

Insert Figure 6 about here

The entries in each cell of the matrix reflect the conflicting evidence received from a source-type in that period. The matrix provides a simple yet powerful way to look at a wide variety of inference problems. In particular, by focusing on source-types (rows) or time periods (columns), one can look at the combining of information either longitudinally, cross-sectionally, or

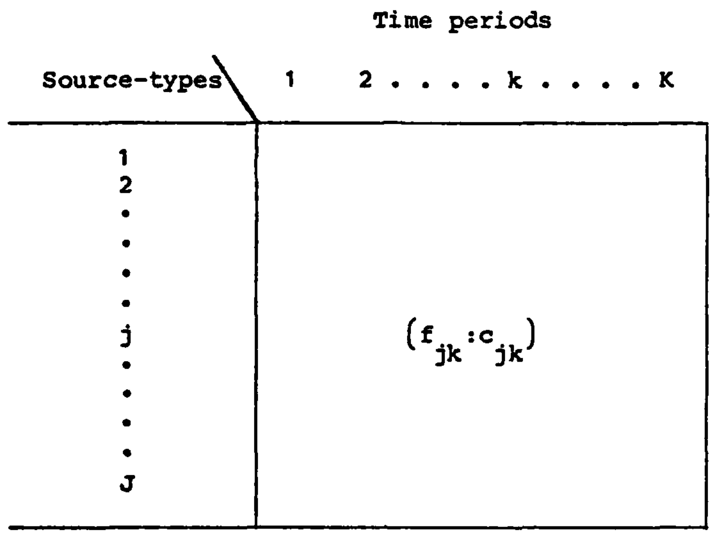

Figure 6. The evidence matrix

both. Furthermore, the issues of reliability and ambiguity become quite complex here since there can be differential source reliability, varying numbers of reports per source, and the sources may not be "independent." While the challenge of understanding how people incorporate such factors into their judgments is formidable, the complexity of inferences in real world settings requires that attention be paid to these issues.

CONCLUSION

In considering the role of ambiguity and uncertainty in inferential judgments, we have developed a quantitative model that accounts for much existing data as well as our own experimental findings. Furthermore, we have shown how this model relates to Keynes' idea of the weight of evidence, the non-additivity of complementary probabilities, risky choice, and current work on cognitive heuristics. Moreover, since inference involves "going beyond the information given" (Bruner, 1957), an important way do this is to construct, via imagination, "what might have been" or "what might be." Such constructions, whether the result of a cognitive simulation process as proposed here, or more elaborate processes (as in resolving surprise), pose an interesting and important trade-off for the organism. On the one hand, there are costs of investing in imagination; increased mental effort and the discomfort that results from greater uncertainty. On the other hand, the benefits of considering the world as it isn't, protects one from overconfidence and its nonadaptive consequences. Thus, finding the appropriate compromise between "what is" and "what might have been" (or, "what might be"), is central to inferences under ambiguity and uncertainty.

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FOOTNOTES

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[1] At $p_A = 0, 1$ there is no ambiguity. Hence, the relation between p_A and $S(p_A)$ should be discontinuous. Indeed, the lack of ambiguity at the end points provides a rationale for the discontinuity of the decision-weight function and this implies the "certainty effect" of prospect theory (i.e., the value of sure gambles is heightened either positively or negatively).

[2] A listing of the program is available from the authors.

APPENDIX A

This appendix considers the effects of different assumptions concerning the weights given to greater and smaller values than that observed. In equation (5), differential weighting is achieved by the β parameter; i.e., $k_g = \Theta(1-p)$ and $k_s = \Theta p^{\beta}$. However, one could also consider linear weighting schemes where the weights given to Θp and $\Theta(1-p)$ sum to one (i.e., a weighted averaging process), or where the weights do not sum to one. For the former, let

$$\begin{aligned} k_g - k_s &= \Theta w(1-p) - \Theta(1-w)p \\ &= \Theta(w-p) \end{aligned} \tag{A.1}$$

where $0 \leq w \leq 1$ is the relative weight given to greater values. Substituting (A.1) into equation (8), we obtain,

$$S_1(f:c) = p + \frac{\Theta}{n}(w-p) \tag{A.2}$$

where, $S_1(f:c)$ is used to denote alternative model 1. Note that in this model, $S_1(f:c)$ is regressive with respect to p. Although this model has appealing features, it is easy to show that it does not capture some aspects of our model and data. Specifically, it always predicts additivity of judgments of complementary events, i.e.,

$$\begin{aligned} S_1(f:c) + S_1(c:f) &= p + \frac{\Theta}{n}(w-p) + (1-p) + \frac{\Theta}{n}[(1-w) - (1-p)] \\ &= 1 \end{aligned} \tag{A.3}$$

However, non-additivity will occur if the weights accorded to $\Theta(1-p)$ and Θp do not sum to one. A special case of this model, which we denote $S_2(f:c)$, and which is similar to the $S(f:c)$ model used in the paper, is one where,

$$k_g - k_s = \Theta(1-p) - \Theta mp \quad (m > 0) \tag{A.4}$$

This yields,

$$S_2(f:c) = p + \frac{\theta}{n} [1-p - mp] \quad (A.5)$$

such that the additivity conditions are,

$$S_2(f:c) + S_2(c:f) = p + \frac{\theta}{n} [1-p - mp] + (1-p) + \frac{\theta}{n} [p - m(1-p)]$$

$$= 1 + \frac{\theta}{n} (1-m) \quad (A.6)$$

Thus, for $m > 0$, the model predicts sub-additivity; for $m = 1$, additivity; and for $m < 0$, super-additivity. The difference between $S_2(f:c)$ and $S(f:c)$ is that the former predicts a constant amount of non-additivity irrespective of the value of p. In the $S(f:c)$ model, the level of p affects the amount of additivity. This is shown in equation (9), which is reproduced here for convenience,

$$S(f:c) + S(c:f) = 1 + \frac{\theta}{n} [1-p^{\beta} - (1-p)^{\beta}] \quad (A.7)$$

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